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On Vaccination. By WILLIAM SHEARMAN, M. D., Member of the Royal College of Physicians, and President of the Medical Society of London.

THE occurrence of small-pox after vaccination appears, of late, to be much more frequent than was formerly found to be the case; and it is very difficult to account for the fact. Ever since the original discovery of vaccination, we have occasionally heard of small-pox taking place in individuals subsequent to that process; but the instances were at first rare, and I have been disposed to attribute some of these to imperfect vaccination, either from a spurious virus having been employed, or from some inattention on the part of the inoculator, or neglect of the patient or his friends. Vaccination is an extremely delicate operation, and requires the greatest nicety of observation to ascertain its complete effect on the constitution. Inspection of the pustule during the whole period of the process is essentially necessary, if we wish to satisfy ourselves of the security of the patient; for as the disease goes on with different degrees of rapidity in different individuals, the stated periods of inspection usually prescribed cannot, in all cases, be sufficient to insure a perfect knowledge of the regular progress of the disease; and there is reason to believe that many persons have been pronounced secured by vaccination, merely from the inoculator observing, at the end of eight or nine days, perhaps, after the insertion of the

virus, a fine full vesicle, without his having watched the preceding periods, so as to ascertain those distinctive characters which are essential to the success of the operation, and without which the mere fluid in the vesicle is altogether an insufficient characteristic. But making every allowance for imperfect vaccination from both of the above causes—spurious virus and inattention to the progress of the pustule—it is scarcely to be doubted that small-pox after vaccination is now a frequent occurrence.

Two instances, in immediate succession, have lately fallen within my own observation. I was sent for to see a young gentleman in Westminster School, the son of a medical friend residing in the country. He had been ill for two or three days with smart symptoms of fever; and when I saw him, he complained of great pain in his back and all his limbs. He had not been altogether confined to bed; for having a part assigned to him in the Westminster play, which was soon to be acted, his anxiety on that account had induced him to attend the rehearsal, although scarcely able to support himself during the requisite period. He was, however, at length obliged to yield; and on the morning of my first seeing him, I found his face and different parts of his body covered with a variolous eruption, amounting, in the whole, to about three or four hundred. He had been vaccinated during infancy by his father. I removed him to my own house; the symptoms of fever gradually subsided; a great many of the pustules were in a short time filled with a fluid, which however, did not go on to perfect suppuration, but they dried up and became hard and horny about the seventh day from their first appearance.

I had residing at home with me, at the time I removed this youth to my house, two daughters, one of whom I had vaccinated about twenty years ago; the other had been vaccinated about fourteen years. The former was seized on the Thursday following, exactly a week from the time of exposure to the variolous infection, in the middle of the day, with an evident cold chilly fit, accompanied with headach and violent pains in the back and limbs, and all the symptoms of perfect fever. On the following day the paroxysm was repeated, but in a much milder degree; on the Saturday the accession was considerably more violent than on the day before, resembling exactly the former one of Thursday. On the following morning several pimples of small-pox appeared on the face and other parts, in the whole about a dozen; the pyrexia left her, and the eruption, assuming the usual hard and horny ap-

pearance, was of no further inconvenience. The younger daughter was not in the slightest degree affected.

After the repeated experience I have had of cases similar to these, I can no longer consider vaccination as a certain preventive of small-pox, although I am very ready to give it credit for considerably modifying the progress and event of that complaint. In the majority of instances, it undoubtedly secures the individual from that disease; and I think we should not greatly err, were we to place the value of this process, when it does not eradicate the susceptibility to variola, on an equal footing with inoculation for the small-pox; it rendering, like that operation, the disease milder in its progress and safe in its event, having rather the balance of advantage in its favour, by its checking the progress of suppuration in the pustules, and obviating the inconveniences of secondary fever. But in this view of the matter, one disadvantage attends vaccination; the individual may be exposed to take casual small-pox under circumstances in which we should carefully avoid subjecting him to it by voluntary inoculation. The season of the year, the present state of health of the party, or some peculiar temporary condition, as pregnancy, may be unfavourable to the safe reception of the infection, and such as we should by no means choose for the voluntary communication of it by inoculation, although it may be out of our power to prevent the individual being exposed to the infection. In cases like these, vaccination, when it has not destroyed the susceptibility to variola, becomes a disadvantage, because, but for the confidence placed in that operation, variolous inoculation would have been resorted to under circumstances the most favourable for its employment.

It is not always possible, in cases of natural small-pox, to ascertain the source of infection; but previous to the discovery of vaccination, sufficient experience had been attained to establish the fact, that a period of twelve or fourteen days usually elapsed, in the casual small-pox, between the reception of infection and the commencement of the eruptive fever; whilst the intervening period between infection and the fever, in inoculated small-pox, was found to be considerably shorter, generally being about seven days only. A great practical advantage was often taken of the knowledge of this fact; and inoculation has frequently been employed two or three days after casual infection, with a view to intercept the effect of the latter, and that with complete success. In the case of casual small-pox given above, the interval was precisely seven days; confirming, it may be presumed, the resem-

blance between inoculated small-pox and the casual disease after vaccination, which I have stated to be probable.

Although vaccination in so singular a manner modifies the progress and event of small-pox, it does not appear, in the least degree, to diminish the infectious nature of this disease, it being as easily communicated by an individual who has passed through that process as by any other person; neither does vaccination appear, in the slightest degree, to diminish the susceptibility to small-pox, unless it has entirely destroyed it. In the short history given above, the subject communicating the infection and the person receiving it had both been vaccinated, and yet the disease appeared in the latter at as early a period as could have taken place, if vaccination had not been employed by either; the disease itself was modified in both instances; the facility of communicating and receiving the infection remained the same.

The deaths which have taken place from small-pox after vaccination have certainly been extremely few; but as, in those cases, the individuals were exposed to the casual small-pox, under circumstances much more unfavourable to the reception of the disease than would have been the case had variolous inoculation been employed, these persons, may, in one sense, be said to have fallen victims to vaccination.

If the supposition be correct, that casual small-pox after vaccination resembles pretty nearly, in kind and degree, that state of the disease which would have followed variolous inoculation in the same individual, if vaccination had not been employed, it may be said that these fatal cases would equally have occurred under inoculation, had that operation been performed instead of vaccination; but it must be recollected, that had variolous inoculation been trusted to, instead of vaccination, care would have been taken to communicate the disease under the most favourable circumstances, and the usual means of preparation would have preceded the operation. As the casual small-pox is in every instance rendered considerably milder by previous vaccination, there is every reason to suppose that this operation would exert the same beneficial influence on inoculated small-pox; hence vaccination, if not a certain preventive of this disease, may, at least, be advantageously made use of as a preparation for inoculation. The deaths under inoculation, as at present managed, are very few; and it is not unreasonable to expect, that, employed after vaccination, this operation would be divested even of the small degree of hazard which at present attends it. In by far the greatest number of those vaccinated, the suscepti-

bility to small-pox is completely eradicated; to these, inoculation would, at the worst, be only a superfluous operation; whilst, in those who were still liable to small-pox, that disease would be communicated in the mildest degree in which it is possible for them to undergo it.

If the fact be well-founded, that the recurrence of small-pox after vaccination is much more frequent now than formerly, it is plain, that unless the cause of this greater frequency be ascertained and obviated, the ratio of insecurity may go on increasing, until, in time, vaccination will no longer be a preventive of small-pox, and can only be employed as a preparation for inoculation. Even in this extreme view of the case, the discovery of vaccination will have been an important benefit conferred upon mankind; and we surely ought to avail ourselves of all the advantages to be derived from the discovery, notwithstanding they may fall somewhat short of what had been too sanguinely expected by the original supporters of vaccination.

Since writing the above, I have seen, in the Edinburgh Review for November 1822, an analysis of Dr. Thompson's Treatise on the Varioloid Epidemic which prevailed in Edinburgh. In this article is the following passage:—"During the prevalence of the varioloid epidemic, Dr. Thomson had great occasion to doubt whether or not the chicken-pox and the small-pox were separate diseases; and his doubt has ended in a strong conviction that they both originate from one common contagion, and that chicken-pox is nothing more than a variety of small-pox." The fact adduced by Dr. Thompson in support of his conclusion, is the interchangeable and indiscriminate succession of small-pox and chicken-pox in different persons infected from one common source of contagion; the first person infected being attacked with small-pox, the person deriving infection from him shewing the chicken-pox, the next the small-pox, and so on, without, however, any regularity of alternation.

The facts appear to me extremely questionable; and I am more inclined to suppose Dr. T. mistaken in the precise nature of the eruption in these individuals, than to admit the inference he adduces. It is well known that a very severe case of chicken-pox so much resembles, in its attack, progress, and appearance of the pustules, a mild case of small-pox, that it is extremely difficult to distinguish one from the other; and sometimes the only certainty we possess of the true nature of the disease before us, is a knowledge that the patient has pre-

viously passed through the other disease. But resemblance is not identity, and the following difficulties oppose themselves to the opinion of chicken-pox being only a variety of small-pox.

If this opinion were true, the variety in question ought to be governed by the same laws as the other acknowledged varieties of true small-pox; but this is not the case. An individual who has once passed through one of these acknowledged varieties is completely protected from an attack of any other variety. A person having had the distinct small-pox, cannot be affected with the confluent small-pox, nor is one having had the confluent small-pox liable to be attacked with the distinct small-pox, while each of these persons is afterwards liable to take the chicken-pox; and a person having had the chicken-pox is still liable to be affected with distinct or with confluent small-pox. If all these three are identical, and not separate diseases, how is it that two of the varieties should be a protection against each other, whilst neither of the two is so against the third, nor that third a protection against the other two? Vaccination, in most instances, protects the individual against both the distinct and the confluent small-pox, but is no protection against the chicken-pox. In those cases where vaccination fails to destroy the susceptibility to small-pox, it undoubtedly so modifies subsequent small-pox, as to render it a much more mild and safe disease than it otherwise would have been; but vaccination exerts no modification on subsequent chicken-pox, so as to render that complaint milder. These are strong facts against the probability of chicken-pox being merely a variety of small-pox, and support, I think, the inference I have drawn, that those cases arising from variolous infection, designated chicken-pox by Dr. Thompson, were, in fact, small-pox in one of its mildest forms.

So far from chicken-pox being rendered milder by previous vaccination, I am impressed with a belief that this disease is, in general, more severe in its attack, and more nearly resembles mild small-pox, in those individuals who have been vaccinated, than in those who have never passed through that process: it is no wonder, therefore, that as vaccination is so general, and, for the most part performed at so early an age, varicella should commonly appear with more of a varioloid character now than formerly; and it is probably this circumstance which has led to the opinion of varicella being but a variety of small-pox.

Until varicella conforms to the laws of the acknowledged

varieties of small-pox, in affording protection against the attacks of all the other varieties of that disease, in the same manner as variola discreta and variola confluens do against each other, I shall continue to consider them as distinct diseases, although I confess myself totally unable to comprehend why vaccination should render one of these diseases milder and the other more severe; an opinion I have formed from my own individual experience, but which I should wish to see established or refuted by the concurrent testimony of Practitioners in general.

II.

(From the Edinburgh Medical and Surgical Journal.)

1. *Observations on Curvatures of the Spine.* By James Macartney, M. D. M. R. I. A. F. R. S. Professor of Anatomy in the University of Dublin.
2. *On the Nature and Treatment of the Distortions to which the Spine and the Bones of the Chest are subject. With an Inquiry into the Merits of the several Modes of Practice which have hitherto been followed in the Treatment of Distortions. Illustrated by Plates in Folio.* By John Shaw, Surgeon and Lecturer on Anatomy.
3. *Pathological Observations on the Rotated or Contorted Spine, commonly called Lateral Curvature, deduced from Practice.* By Andrew Dods, M. D. late of Edinburgh, and Surgeon in the Royal Navy.

Personal deformity, whatever be its origin, is a subject on which the human feelings are so strongly and irresistibly interested, that every attempt on the part of the pathologist and surgeon to explain its causes, and counteract their effects, wil naturally be received with considerable attention. Those varieties, which depend on changes in the figure, position, ana direction of the bones of the vertebral column, are invested with circumstances which render them more strongly than others the object of inquiry to the surgical practitioner. Their connexion, in the character of protecting walls, with a great and important part of the central nervous mass, the support which they afford to the whole trunk, to the muscles which move it, and the upper extremities, and the necessity of their healthy position to the functions of the organs of respiration, and of digestion, all combine to render the study of the causes of vertebral deformity an object of very great importance to all

orders of the medical profession. The several works, the titles of which are above enumerated, prove, that if readers abound, writers are not wanting; and this number even might be augmented by the addition of some foreign authors. It is our intention, however, to confine ourselves exclusively to those now before us, for two reasons,—first, because most of them treat of the same species of spinal deformity,—and, secondly, this is quite sufficient for present consideration.

The first of these is from the pen of an eminent anatomist, Dr. Macartney, of Dublin. It is exceedingly short; and perhaps the author has not entered with sufficient minuteness into the pathological causes of spinal distortion, or given the subject that elaborate consideration which his opportunities as an anatomical teacher, would lead his readers to expect. His observations on the effects of different modes of treatment are numerous and judicious.

The second work, by Mr. Shaw, of London, is a much more elaborate performance, and contains a great deal more information. The author introduces his work with observations on the pathological causes of distortion, in which he considers the effects of inaction, on the nutrition and structure of the animal tissues in general, and of bone and muscle; and afterwards the effects of exercise, in increasing the size and power of the muscles, and augmenting the strength and density of bones. This is succeeded by an inquiry into the causes of the partial palsy and wasting of one of the limbs during infancy, and its influence in producing distortion of the spine. He then proceeds directly to the proper subject of consideration,—that of distortion, commonly called *lateral curvature of the spine*. He enumerates the changes in personal figure and appearance which take place at its commencement, inquires into its immediate causes, and concludes this part of the subject with a particular description of the state of the shoulders and ribs. Mr. Shaw then inquires into the merits of the several opinions which have been entertained on the causes of lateral curvature, whether it proceed from irregular action of muscles, from disease of the ligaments, or from dislocation of the vertebræ, or laterally, from disease of the vertebræ. In the three subsequent chapters he illustrates the varieties of distortion, by explanation of the engravings published in the folio work; and in explaining the figures of the fourth engraving, shows that the spine may be distorted to the greatest degree without proof of the existence of rickets; that the pelvis, in such cases, is never affected, and is not liable to be distorted by the use of instruments. The subject is concluded by a

section or chapter of observations on the effects of spinal or thoracic distortion on the internal organs. This may be said to terminate the semeiographical and pathological part of Mr. Shaw's work. The subsequent part is almost entirely on the treatment of the varieties of spinal distortion, on the merit of the different modes of treatment that have been proposed or practised by surgeons, by bandage-makers and machinists, and by downright quacks. The last part of the volume consists of an Appendix, in which Mr. Shaw gives short descriptions of the most conspicuous characters of ninety-four specimens, either of distortion, or of disease, or of injury of the vertebral column, which are preserved in the anatomical collection of Mr. Charles Bell.

Such is a general outline of the contents of Mr. Shaw's work. Of the merit of these contents we will speak presently, or at least our readers will judge best when we come to enter on the subject more particularly. It is enough now to say, that the volume contains a great deal of information of the most satisfactory kind to the pathologist and practitioner; for it comprehends a very considerable number of facts, and its principles and inferences are never made without the evidence of strong and substantial facts, derived chiefly from the examination of instances of spinal distortion. It is to be regretted, however, that the arrangement of the work is most provokingly defective. Mr. Shaw has a decided aversion to the artificial distinction of section, chapter, or subsection, so useful both to author and to reader; and has delivered all his information in the form of independent short essays, which are not in every instance connected with each other in the most obvious way. Had these essays been numbered even, this inconvenience would at least have been diminished. In its present state, it is impossible to tell, without enumerating them, of how many of these essays the volume consists. They are generally preceded by something like a title; but this does not always appear in the same form;—in not a few we find a general aphoristic proposition at the commencement of every essay, the object of which is evidently to adduce the proofs and arguments on which this proposition rests; while in others, one title is fixed at the beginning, and another appears in the table of contents. Notwithstanding all these objections, however, which certainly render it no easy matter to collect all our author's information, and which will undoubtedly render it difficult to consult, we have no hesitation in saying, that it is the best and most instructive work hitherto published on the subject of spinal distortion.

Of the engravings which accompany, or rather which have followed it, it will be perhaps expected that we say something. They consist of seven folio-plates, and five etchings in the letter-press pages. The three first plates represent various forms of spinal distortion, in one or two instances in the living subject, but chiefly in the skeletons of persons so deformed. The fourth exhibits on instance of distorted chest; the fifth the usual appearances of the adult rickety skeleton. The sixth contains various figures of machines, collars, spine-bandages, and supporting-stays; and the seventh exhibits figures showing the author's manner of preventing and counteracting spinal distortion. As works of art, it is scarcely requisite to express any opinion of the merit of these engravings. They are mostly done by the author, and are creditable specimens of his talents as a delineator.

The third work in our list, by Dr. Dods, consists of four general divisions or sections. In the first, the author delivers introductory observations on the *rotated*, or *contorted spine*,—a term which he adopts in preference to *lateral curvature*, to designate that species of deformity in which the vertebral column deviates from its natural direction, not by the antero-posterior bend, or gibbosity, but by a lateral or oblique twist. In the second, he inquires into the causes of this kind of deformity, which he thinks are entirely referable to irregular action of muscles. In the third, he inquires into the manner in which this rotation or contortion of the spine is produced, and explains the reasons which have led to its being mistaken for incurvation of the spinal column. The fourth and last section,—on the prevention and cure of rotated spine, is subdivided into two parts,—the first, on the prevention of this species of deformity in its incipient and unformed state; and the second, on its cure and treatment after it has taken place, and proceeded to some length. In the former, after inquiring into the propriety of various mechanical contrivances employed by persons superintending the education of youth, such as education-chairs, stays, crutches, collars, steel-boddice, back-boards, stocks, reclining-beds, and condemning all of them as cruel and useless measures:—he concludes with recommending school-drilling under a drill-sergeant, with proper restrictions, as the most efficacious mode of preserving the symmetry and elegance of the person. In the second part, when the author treats of the cure of the deformity after it has taken place, he again considers the merit of mechanical means, and again reprobates them in strong terms of disapprobation; adverts to the treatment by issues introduced by Mr. Pott, but re-

stricts it only to distortion depending on disease of the vertebræ ; inquires into the merits of the plan suggested by the late Mr. Grant, of carrying weights on the head, and defended by Mr. Wilson, and, for at least four reasons, concludes it to be in every case of contorted spine injurious in the extreme ; and finally, winds up the whole subject with an account of the practice which he recommends in conformity to his pathological principles,—by friction, manipulation, position and exercise ; on each of which subjects he dwells at some length.

We have given this short sketch of the contents of our two most elaborate works, not as an analytical account of the researches or opinions of the authors, but merely to communicate some idea of the subjects treated, and of the order in which they are discussed. A minute abstract of each work, however condensed, would have necessarily led to repetition, and in consequence of the difference of arrangement, to some confusion. We have also refrained from any notice of the comparative merits or defects of the works, because it is not consistent with justice to consider them in a general way. We now proceed to lay before our readers what will be much more advantageous to them, without being injurious to our authors,—an account of the subject itself, as it is substantially treated in their writings, but in a mode of arrangement more suited for perspicuity, as we think, and better adapted for the purpose of enabling them to distinguish the certain and the doubtful points of a practical inquiry, in which very different opinions are entertained by different authorities.

It is probable that the bones of the vertebral column are liable to be deformed or distorted in various modes ; and the sources of these kinds of distortion are very different. Some sorts of deformity are known to be coeval with existence, or congenital ; others are known to come on a few years after birth ; and others have been known to occur after adult age. When, however, they are to be distinguished by the peculiar direction which they take, all the varieties which have been hitherto described may be referred to two ; the first, the *antero-posterior or angular curvature ;* the second, that termed by Mr. Shaw the *lateral curvature*, and by Dr. Dods, *Rotated* or *Contorted spine* In the former, a bend more or less considerable takes place at some individual point of the column ;—the upper division is bent or incurvated on the lower ; and a convexity more or less considerable takes place on the posterior surface of the column. The process has been termed *incurvation*, the state of the spinal column has been said to be *incurvated*, and the deformity to which it gives rise has been

termed *gibbosity*, or popularly *humpback*. Of the latter form of spinal deformity, it is not quite so easy to give any thing like an accurate or unobjectionable definition. Though both our authors describe the appearances of distorted persons at sufficient length, yet we find no attempt at a definition of the exact nature and kind of deviation from its healthy and natural direction, which the spinal column undergoes. In the absence therefore of such a definition from more able hands, we beg to submit the following, to enable our readers to comprehend distinctly what is meant by the opposite terms of *lateral curvature* and *contorted spine*. The vertebral column is said to be in a state of lateral curvature, when it is so much bent from the mesial plane to the one side, that a vertical plane would not divide it into halves, but would form sections oblique or transverse of several of the individual *vertebræ*. If this definition be compared with examples of skeletons of this peculiar variety of deformity, it is obvious, we conceive, that it will include every possible case in which the vertebral column deviates from its natural direction.

The first variety of spinal deformity occupies very little of the attention of either of our authors; and we therefore cannot say that we enjoy much of the benefit of their instruction.—The light in which its consideration is most interesting, is the inquiry of its nature and origin, and what peculiar state of the vertebral bones or their ligaments gives rise to this change of shape. Since the time when Mr. Pott published his observations on palsy of the lower limbs, it has been generally understood that its most frequent and common cause was chronic inflammation of one or more of the vertebræ terminating in ulceration. According to the account of this eminent surgeon, "in infants and young children, who had been afflicted with the disorder but a small space of time, the ligaments connecting the vertebræ, which formed the curve (the articular capsules, &c.,) were in some degree altered from a natural state, by being somewhat thickened and relaxed, and the bodies of those bones were palpably spread and enlarged in their texture, just as the bones forming the articulations are in children who are called rickety." "In those" again, he adds, "who had long laboured under the distemper, and in whom the symptoms were aggravated, whatever might be their age, the ligaments were still more thickened, relaxed, and altered, the bodies of the bones more spread, more enlarged, and more inclining to become carious, and the cartilages between the bodies of the vertebræ much compressed and lessened in size; and lastly, he concludes, in all those who had so long labour-

ed under the disease as to have been destroyed by it, or by its consequences, that the *corpora vertebrarum* were completely carious, the intervening cartilages totally destroyed, and a quantity of *sanies* lodged between the rotten bones and the membrane investing the spinal marrow."—*Pott's Works, Vol. III.*

When we compare this description of the morbid changes which are here incident to the bones and ligamentous tissues of the spine, with the account which the author gives of the deformity or incurvation which results from it, we learn that the morbid process now mentioned certainly produces one species of deformity, but that it is uncertain whether it causes others; and that, undoubtedly, other species of deformity may take place, not from this, but from other causes. To assist us in forming an accurate distinction, it is indispensable to attend to the language which Mr. Pott himself uses. Among the circumstances which he has noticed, attending the appearance of this disease, three are particularly remarkable. The first of these is, that none of the strange twists and deviations which many European women get in their shape, however great the deformity might be, ever caused any palsy of the limbs as the disease in question did; the second is, that neither the degree nor extent of the curvature made any change in the degree of the paralytic symptoms; and the third, which is particularly important, is, that the curvature attending this malady is almost always from within outward, and seldom or never or either side.

In the course of his subsequent experience, Mr. Pott took occasion to modify several of his pathological opinions. He no longer regarded the loss of power in the limbs as palsy, but as a sort of cataleptic stiffness, arising, as he thought, from spasmodic action of certain muscles, and from inability to move others, by which, during health, their action was antagonized. Though he still continued to consider the disease as a distempered or morbid state of the bones composing the spine, or of the parts by which they are connected, repeated inquiry and examination had convinced him, that the affected bones were *not enlarged and spread*, as he first stated, but *rather eroded* or consumed by *caries;* that the bodies of the vertebræ were invariably affected with this erosion; and that they were affected in many cases in which the ligaments bore but little mark of distemper. Mr. Pott further came to the conclusion, that the state of the intervertebral cartilages in this disease was various,—being sometimes totally destroyed while the bones were little affected by caries, and in other cases lit-

tle injured where caries was considerable and extensive; that in some instances in which the ligaments and cartilages become the seat of disease, without affection of the vertebræ, the whole spine may give way on one side, and form lateral curvature; and that this disease, whether it affects the bones, the ligaments, or the cartilages, or appears in the form of purulent cysts, always depends on the same disposition which gives rise to the symptoms of scrofula.

Since the days of Pott, this disease has become the subject of investigation to several authors, among others, very shortly after to Dr. Jebb, and to Sir James Earle, and more recently to Mr. Baynton of Bristol, and Mr. Thomas Copeland of London. But to none perhaps are we more indebted for a clear statement of its origin and progress and nature, than to Giovanni Baptista Palletta, late Professor of Anatomy and Surgery in Milan, and to G. B. Monteggia of the same city, the learned author of one of the most comprehensive systems of surgery that has appeared in modern times. Palletta informs us,—that "in the commencement of the disease, the ligaments of the vertebræ become thicker and more lax, the bodies of the vertebræ scarcely swell, the intervertebral cartilages are compressed and softened; in short, as the evil increases, such loss of these parts takes place, that the cartilages and ligaments being destroyed, the bodies of the vertebræ are found more or less consumed, and the column gradually becomes gibbous. The ligamentous band which connects the bodies of the vertebræ, is distended into a sac by the contained matter, which may be sanious, or thick like suet or cheese, with scales of wasted bone floating in it. This disease often affects several vertebræ at once, and spreads to the ribs, the heads of which have been repeatedly found corroded, and detached from the vertebræ." All or most of these evils, Palletta justly ascribes to inflammation, either attended with manifest signs, or latent and insidious—whatever be the remote cause; and declares, that he has been so much confirmed in this opinion by recent examples of the disease, that he felt himself compelled now to retract in the most solemn manner whatever he had previously asserted to the contrary.

We learn from Monteggia, who had examined with Palletta several instances of this disease, that they found the same changes as Pott did, in the substance of the vertebræ, and in the intervertebral cartilages; and Monteggia justly remarks, that the knowledge of these changes explains the deformity (*cyphosis*,) which succeeds in a satisfactory manner. "As the bodies of one or more vertebræ," says Monteggia, "or several

intermediate cartilages, are wasted, the upper contiguous vertebra, being deprived of support, is forced to fall downward, and to sink into the space left by the wasted bones. Hence the spine is bent forwards, and the spinous processes of the annular portion, which the disease has not yet reached, are proportionally elevated. The seat of the wasted vertebræ is then observed to be covered anteriorly by a tumour formed by the distension of the anterior ligamentous covering of the bodies of the vertebræ, under which is found a thick, white, pulpy mass, which fills up the vacuity of the osseous destruction. In short, this vertebral tumour is very similar to an atheroma or steatoma. And as the *cyphosis* of Pott is often on the back, so the internal tumour on the bodies of the vertebræ is often accompanied with analogous tubercles on the posterior part of the pleura and lungs."—*Istituzione Chirurgiche, Vol. V.*

Inflammation of the vertebræ or vertebral fibrocartilages, and consequent ulceration, is however only one cause of the antero-posterior deformity. The morbid changes, which are thus induced in the bones and their connecting tissues, give rise to incurvation ; but they also produce palsy or cataleptic rigidity of the lower extremities. Two questions are here naturally suggested ; first, Can this disease take place to any extent without being followed by palsy—without affecting the spinal chord or the nerves which issue from it below the seat of disorganization ? and, secondly, What is the particular description of cases in which this incurvation takes place without affecting the motions of the limbs ; what changes are induced in the bones, or their connecting ligaments, sufficient to cause incurvation, without at the same time affecting the spinal chord or its nervous productions !

On the first of these points there is evidence to believe that a certain degree of the morbid action may occur sufficient to cause incurvation, yet without affecting the muscles of the limbs. Monteggia informs us, that he had met with examples of Pott's disease, (chronic inflammation of the vertebræ,) in which the morbid process was so far advanced that the usual remedies could not, or did not act on it ; yet the patients began to improve of themselves, acquired distinct strength in their limbs, and began to walk with, or even without crutches,—which they could not previously do. This improvement he ascribes to the circumstance of the disease becoming after a certain time stationary, to the probable solution of the swelling, the absorption of the pultaceous matter, and the cessation of what he calls the *ossivorous* action. Benjamin Bell observ-

ed a similar instance of the spontaneous disappearance of palsy, and ascribes it to the disease extending over a greater number of bones, whence Monteggia thinks the spine would form a bend less acute and less injurious to its nervous chord. Hull also records an example of a paraplegic case, in which the palsy disappeared as the gibbosity or hump increased in size, when it included a greater number of vertebræ, and made an angle less acute. These facts, which might be increased considerably by more extended research, show that incurvation, arising from vertebral disease, may take place without permanent palsy, and consequently without permanent injury to the spinal chord. It is indeed not difficult to conceive this disease taking place so slowly, and being at the same time so completely confined to the bodies of the vertebræ and the intervertebral cartilages, that the incurvature shall not be either so sudden, or so abrupt and angular as to injure the chord.—It appears from the results of many cases, that the sudden or rapid progress of the disease, and the rapid incurvation which succeeds it, is the great cause of injury to the chord sufficient to induce palsy.

But although we have adduced these authorities with caution, and without speaking confidently on a point in which we are desirous to convince our readers by proofs which, though undoubted, are presented to their understanding deliberately, this conclusion, which we have now stated, does by no means rest on uncertain ground. One of the first cases of chronic inflammation of the vertebræ terminating in ulceration which we witnessed, was an example in which two dorsal and one lumbar vertebræ were affected to such an extent as to diminish the stature of the individual considerably, and to cause a distinct incurvation of the spine and projection of the back; yet the motions of the legs were not impaired farther than by general weakness. Since that period, we have remarked the same disease more than once without the effect of paralyzing the limbs; and particularly in one case of lumbar abscess, the bones were much injured, and incurvation was considerable ere palsy came on, which was only a few days before death. We state these results of our own observation and experience, not because we believe them to be singular, but because they are most accessible, and were the first which led us to doubt the justice with which Pott generalized his inference on the vertebral disease being uniformly attended with *paraplegia*. We have no doubt that many similar instances have fallen under the notice of our surgical friends of any standing in the profession. But if further evidence on the

question were requisite, we have much pleasure in giving the following from an anatomical inquirer like Mr. Shaw.

"No. 49, is a specimen of the carious state of the vertebræ, which precedes the destruction of their bodies, and the consequent yielding to the superincumbent weight.

"While the disease is (as in the case from which this was taken) only commencing, there is generally paralysis of the legs. This is probably in consequence of the inflammation of the bone affecting the spinal marrow; as many facts could be adduced to show, that the idea generally received, of pressure on the spinal marrow being the cause of paralysis in such cases, is erroneous. A specimen, preserved in the museum of St. Bartholomew's Hospital, is particularly interesting, as having been taken from a patient who had been kept for some time in the Hospital by Mr. Pott, as an example of the good effects of the caustic issues upon caries of the spine with curvature. Although the patient had been cured of paralysis, and was able to walk for some time preceding his death, yet it was found, upon dissection, that the spinal marrow had actually been diminished in size by the growth of the parts in the interior of the canal.

"Since the death of Dr. Baillie, some drawings, which were found among his papers, have been sent to me. Besides the value which I naturally place upon these drawings as a gift, I consider them highly important, as they afford the best evidence on the present question.

"One of them represents a spine more distorted than that given in the sixth figure of the first plate. The canal for the spinal marrow, is at one part so much encroached upon by the projection of the bodies of the vertebræ, that it is scarcely half its natural size; the spinal marrow consequently appears compressed or diminished.

"The patient, from whom this was taken, had been attended by Dr. Hunter, who used to say, that had he not carefully watched every symptom of the disease, he should have thought, from the state of the preparation, that all the parts below the point where the canal was diminished, must have been paralyzed. The circumstance of there having been no paralysis, is proved beyond a doubt, by a note on the drawing, in Dr. Baillie's handwriting:—"Miss Smith, aged 12. The incurvation began about four years of age. No palsy to the last."

"These preparations prove that the spinal marrow, like the brain, does not suffer much from a certain degree of pressure, if it is not accompanied with inflammation. The numerous

instances in which there is paralyssis, although there is no pressure upon the spinal marrow, and the above examples of the existence of a certain degree of pressure, without consequent paralysis, afford a satisfactory answer to the severe critique upon Mr. Pott, that is contained in a work published a few years ago. The author of the work alluded to, seems to consider all the symptoms of paralysis, consequent upon diseases of the spine, as always referable to pressure on the spinal marrow.

"In the next volume I shall enter more into this inquiry; but as an opinion prevails, that pressure on the spinal marrow is the cause of the paralysis which is consequent on disease of the vertebræ and injury of the spine, these remarks have been made in the hope of exciting others to the inquiry."

On the information here communicated, we have merely to remark, that it is of no consequence to the question, whether the palsy is caused by pressure, or by inflammation. The chief point is the existence of the disease, and the attendant deformity without palsy of the limbs; and this, we conceive, is unequivocally established. Inflammation existing in nervous parts does eventually compress,—in generally by the products or new formations to which it gives birth; and it will depend on the extent to which this compression is carried, and on the rapidity with which it takes place, whether the organization shall be so far impaired as to suspend the functions of the parts, to which it ministers.

On the second question, the information of most authors is vague and unsatisfactory,—consisting rather of specious *a priori* views, than of conclusions derived from actual examination of instances of incurvated spinal column. It is by all agreed that rickets may be a cause of this deformity; but it is again stated, that when this is the case, the scull, the pelvis, and the cylindrical bones, in general partake more or less of the same deformity. The inquiry, then, is reduced to this;—Are there any states of the vertebræ, exclusive of the rickety condition, which would cause the antero-posterior incurvation of the spinal column? First, says Portal, external causes, as swathing of children, whalebone stays, vicious positions of the trunk and extremities, frequently occasion inflexions of the spine. (*Anatomie Medicale*, I. p. 300.) Next, says Boyer, through Richerand, the young person who is nearsighted, bears the head unusually bent; the vertebral column is bent forwards; and, unless the habit is corrected, the bones harden in this unnatural position, and the neck and head are unnaturally bent forwards. The same thing may happen to

the dorsal or lumbar portion of the vertebral column with delicate young persons, by rapid elongation of the column during a tedious illness. The extensor muscles of the trunk too much weakened, are unable to keep it in the erect position; and hence it falls forwards by the combined weight of the head, the thoracic and abdominal organs. The opinion of Monteggia is much to the same effect. The different incurvations of the spine, says he, are the consequence either of bad habits and attitudes, or of weakness, particularly in delicate young persons, when they grow rapidly after a long disease, or of the rachitic vice, which is most ordinary, and sometimes scrofulous, venereal, rheumatic, or other vice capable of injuring the vertebrae. Early life is a predisposing circumstance, and young girls are more exposed to these deformities than boys; yet Pouteau, Richter and Morgagni saw persons become hump backed in mature age. It is farther known, that decrepitude causes incurvation, in addition to what is elsewhere said on *softening of the bones*, (*malacostea*) and on violent lesions capable of changing its figure at any age. Hump-back is also hereditary. Lastly, Baron Boyer, in his own character, expressed his opinion of the etiology of incurvation to the same effect. Labour requiring constant stooping, the habit of bearing heavy burdens on the head or shoulders, early life with delicate structure and a bad habit, or spasmodic contraction of the muscles, and old age with debility,—all tend, at any age, to bend the vertebral column and cause deformity.

These statements on the cause of incurvation, which we purposely take from some of the first authorities in surgery, prove incontestibly one thing, that the most vague whims are entertained on the subject, and that scarcely one precise or unequivocal cause adequate to account for the effect is assigned. It cannot, with any justice, be said, we conceive, that either swathing, bandages, or stays, or the habit of bending forwards or stooping, are sufficient to cause the marked degree of deformity which is often perceived in the vertebral column. We do not refuse these circumstances some little influence; but it must be small indeed, and, our own experience would say, merely accessory. There must still be some primary and essential cause, the operation of which is general, and more uniform than either of those assigned above. We are unwilling to deviate from our plan of recording facts and the inferences to which they lead; and in the absence, therefore, of well-authenticated cases of spinal incurvation, unconnected with rachitic disease, or vertebral ulceration, it is not

our purpose to indulge in conjecture or supposition. There are, however, two points to which it is material to attend, and with the knowledge of which, it does not require great stretch of belief to think, that antero-posterior incurvation may take place without absolute ulceration of the vertebrae. 1*st*, It is certain that those parts of the skeleton which consist of what is termed spongy osseous texture, such as forms the short bones in general, and the extremities of the cylindrical bones, are, in many persons, very slowly converted into complete firm bone. This slow ossificaion is particularly common in young persons of slender frame and delicate structure; and it is generally in such subjects that incurvation takes place. 2*d*, It is also to be remarked, that in the same kind of subjects the union or consolidation of the component parts of each *vertebra* is not unfrequently very late. The articular processes are often for years separable from the bodies of these bones, and, in the same manner, the spinous processes are sometimes imperfectly ossified, and their plates not united to each other. We do not submit this either as a piece of theory, or as a certain mode of explaining all cases of incurvation. It is a matter of fact in the formation and growth of the skeleton, and does not apply to all, but only to persons of a particular structure and organization. Whether it has any real influence in the production of spinal deformity, we leave to be determined by those to whom frequent opportunities occur.

Of the antero-posterior incurvation in which the gibbosity is anterior, and the depression behind, it is unnecessary to take any particular notice. Its explanation, unless when it is occasioned by injury or violence,—a frequent occurrence, depends on the same principle as that of which we have now been speaking.

On all these points, we are led to expect, from what Mr. Shaw says in several parts of his work, but especially at the 19th page of his Engravings, important and satisfactory information in a second volume, which it is his intention to publish.

The second mode in which the spine may be bent from its natural direction, constitutes what systematic authors have named *scoliosis*, *tortuosity*, or twisting,—a term which appears to have been used by Hippocrates, to express every species of deviation. It is this species of deformity which is the subject of inquiry, under the name of *lateral curvature*, to Mr. Shaw, and of *contorted spine*, to Dr. Dods. The chan-

ges which it induces on the personal figure and appearance, are thus described by Mr. Shaw.

"When the spine of a girl, about the age of fourteen, is becoming crooked, the attention of the mother or governess is at first directed to the state of the shoulders or breasts. At this age, it is most frequently to the latter; one either appearing larger than the other, or growing so unequally as to lead to a suspicion that it is diseased. But in a younger girl, the state of the right shoulder generally attracts attention, as it appears larger than the left, and when the distance of the two shoulder-blades from the spine is compared, it is found to be farther removed than the other; or is, as often said, "*growing out.*" It may perhaps also be noted, that the left hip is different from the right.

"If the spine (which has probably been overlooked by the mother) be now examined, it will be found to be curved nearly in the form of the italic *f*. The whole of the right side will also have acquired a rounded and barrel-like form, while the left is diminished and contracted, the ribs being closer together than is natural. There is, moreover, a sinking in, or depression of the right, and a corresponding fulness of the left loin.

This description nearly corresponds with the sketch of the girl's figure in the second plate given in illustration of the effects produced by a slight lateral distortion of the spine. The second figure in the same plate will assist us in forming a correct idea of the causes of the above changes in the form; for it will at once be evident, that the size of the right shoulder, the swelling of the same side of the neck, and the increased distance of the tip of the shoulder from the head, do not depend on any enlargement of the scapula or muscles of the shoulder, but on the change in the relative position of the ribs. It will also be obvious, that the appearance of the left hip being *out*, is caused by the left ilium being more elevated than the right, which is a consequence of the weight of the body being sustained principally by the right leg. The cause of the barrel-like form of the right side of the chest, and the contracted state of the left, is also obvious; and it is easy to comprehend how the projecting transverse processes of the lumbar vertebræ may give the appearance of fulness to the lower part of the same side. The sketch is so demonstrative that it is scarcely necessary to add, that these changes depend in a great measure on the condition of the spine. This kind of distortion is further described in the explanation of the plate."

This plate contains representations of two objects, a back view of the figure of a person in this variety of distortion, and a similar view of a skeleton to illustrate, or rather demonstrate the change of direction which the bones of the spine and of the chest have already received. In the former figure, the right shoulder is shown to be broader and higher than the left; the upper part of the right side is convex, prominent, and rounded, while the left is compressed and contracted. The expanded or convex form of the ribs on the right side, is a consequence of their rising from the convexity of the curve; while those of the left, which are attached to its concavity, are depressed. The shoulder-blades are also at unequal distance from the vertebral column, that of the right side being nearly double the distance of the left.

"The demonstration afforded by the figures of this plate," continues Mr. Shaw, "makes it almost unnecessary to repeat, that the cause of the "large and high shoulder," on the right side, and of the hip being out on the left, is not in any change that has taken place on the bones of the shoulder, or of the hip, but in the curvature of the spine. The sketches not only prove this, but that the enlargement of the upper part of the right side, depends on the manner in which the ribs project from the convexity of the curve, while the fulness in the left loin is in a great measure formed by the projection and twisting round of the transverse processes of the lumbar vertebræ. The sinking in of the right loin, and the straightness of the line from the left *axilla* to the left hip, are not very distinctly marked here; but the causes on which these changes depend are sufficiently obvious."

After first showing, that, by merely twisting the pelvis of a person whose spine is straight, one leg may be made to appear longer than the other, he proceeds to say, that 'when the spine is distorted, while the individual stands on the left foot, the right leg will appear shorter than the left.'—"That the apparent shortening of the right leg depends on the curve at the loins altering the position of the pelvis, may be demonstrated in two ways. If the patient be laid on the moveable plane, and the shoulders fixed, the curve at the loins may be made straight by pulling upon the lower board. Both legs will then appear of the same length; but if the patient be laid on the carpet, and the curve at the loins increased by pushing the pelvis towards the shoulders, one leg will appear much shorter than the other."

These observations on the changes in the personal shape apply more particularly to one degree of lateral curvature, when

it is yet slight, and in its early stage. As it advances, it gives rise to more conspicuous changes, which Mr. Shaw has enumerated in a subsequent section of the octavo volume, and which should be diligently studied by all who make the treatment of distortion an object of attention. These changes, like the curvature from which they proceed, are in varions degree, and different in kind, at the several periods of the change in the direction of the vertebral column. 1*st*, In such cases as that represented in the engraving, which may be accounted the first degree, the right shoulder and breast will probably be larger and more prominent than the left. 2*d*, If this degree of curvature increase, which may be supposed to constitute a second degree of the change, though the right shoulder continues large and round, the breast of the same side may be flat and contracted, in consequence of the ribs being flattened in front. In some instances, too, it will be observed, that the breast and upper part of the neck on the *left side* are prominent, while the same parts on the right side are lower, though the shoulder-blade is more prominent than the left. 3. In a more advanced state of curvature, a complete change of form takes place, which Mr. Shaw has described in his explanation of the second and third figures of the third plate in the following manner.

"The lumbar vertebræ are scarcely affected, the distortion having fallen almost entirely upon the dorsal, and consequently very much on the ribs. We can easily imagine how much more deformed a person appears with the spine in this condition, than when it is of a serpentine form; for the figure is tall, the difference in the size of the shoulders is very great, and the head, instead of being nearly perpendicular to the pelvis, is directed to one side. On examining the condition of the bones in this example, there can be little doubt of the distortion having been originally similar to that in the upper part of a spine affected with the common lateral distortion. We may therefore suppose, that, at first, the right side was more capacious than the left; but now it is evident that the area of the right side is much diminished. It is highly important to attend to this; for although it may be the consequence of the natural progress of the distortion, still it may also be produced by the misapplication of instruments which have been used with the intention of pushing in the projecting shoulder. It is easy to demonstrate, that the application of an instrument with this intent not only diminishes the area of the chest, but increases the distortion; for the shoulder is pressed against the flat part of the ribs, and consequently the unnatural angle.

which, even in cases of slight distortion is to a certain degree formed in the ribs, is increased."

We believe we have already dwelt sufficiently on the changes induced in personal figure by spinal curvature. We must now proceed to notice the causes to which it has been ascribed by various authors. Mr. Shaw, in discussing the merit of the several opinions which have been entertained on this subject, restricts his examination to three, 1. Irregular action of muscles; 2. Disease of the ligaments, with dislocation of the vertebræ; and, 3. Dislocation of the vertebræ.

1. It is remarkable that Morgagni was disposed to think, that the lateral inclination of the spine arose from weakness or palsy at the part opposite the vicious inclination, in consequence of which it was dragged by the strongest side. To this opinion, Monteggia justly remarks, it was an obvious objection, that no proofs of such lateral palsy existed. The hypothesis, however, of irregular muscular action, being the cause of spinal distortion, has been espoused in one shape or another by many practitioners. It is common in Germany. Baron Boyer, in his recent System of Surgery, speaks of the spasmodic action of the muscles causing distortion; and the opinion has been rapidly gaining ground in London, and other places of this Island. Dr. Macartney, for example, expressly says, that "the third cause of distorted spine is the feebleness of those muscles which are employed in maintaining the erect position of the body. The tendency of a weakened state of the muscles to augment the more serious kinds of curvature, and to induce, even without a softened condition of the bones, an evident distortion of the back, does not appear to have yet received sufficient consideration, either from medical men, or from those whose interest it is early to notice and prevent deformity in young people."

The most zealous advocate for this opinion in modern times is unquestionably Dr. Dods, who, in the Treatise before us, loses no opportunity of inculcating strenuously, that the muscles are the sole agents of evil, in producing contortion or rotation of the spine. We do not say that Dr. Dods is either a very able or a very conclusive reasoner on this subject; for he is one of those practical authors with whom the world at this moment abounds, who can write and declaim about the natural beauty and symmetry of the human body, and execrate the wickedness of those persons who have introduced all the cruel refinements of what is termed a polite and accomplished education. After following him over many flowery pages and fascinating digressions on the popular topics of education,

school-drilling, training of young ladies to a genteel carriage, catching little rustics, as he terms them, and bringing them into shape in boarding-schools, we at length tracked him to the following passage.

"The muscles I allude to are what are generally called *the long extensors of the back*, viz. *the sacro-lumbalis* and *longissimus dorsi*, with the *quadratus lumborum*. It is evident, then, that as these muscles are inserted into transverse levers, they become muscles of rotation as well as extension. For example, if the extensor muscles on both sides of the spine be thrown into equal action at the same moment, the motion produced will be direct extension of the column; but, if they be thrown into action only on one side, the spine, although made to incline in the direction of the muscles' forces to that side, will be moved by means of the rotatory movement of the vertebræ.

"If the extensors of the spine, then, from being inserted into transverse levers (the transverse processes of the vertebræ,) be constituted muscles of rotation, when they act on one side only, it will follow, that should disorganization, producing contraction, take place in those of one side, the effect of this contraction, by its overbalancing the action of their fellows on the opposite side, will be to rotate the vertebræ to which they are attached, and destined to move; and, consequently, to produce rotation or contortion of the column, and not curvature."

These observations, however, which the author with equal truth and modesty calls *cursory*, are merely preliminary to the proper subject of inquiry and exposition. Dr. Dods first reminds his reader of the difficulty of enforcing the obedience of children, and training them in the way they should walk; the anxiety of fond mothers to have elegant and handsome daughters, and the natural emulation of the young ladies themselves; then shows what sacrifices they will make, and to what restraints they will submit to ensure a good figure; and concludes by stating, that the effect of this restraint is to disorganize the muscles, and render them contracted, and so terminate in contortion of the spine.

"In this contracted condition they remain day after day, and night after night (for it may be observed, that their beds are now made to keep the body extended as much as possible,) until the extensor muscles, by this constant action, get disorganized and become contracted; and as the stress more particularly falls upon the muscles of one side of the spine, as

shall be presently shown, so it is that they chiefly suffer this disorganization, and produce the contortion.

"The right arm is that which is generally made use of; and if any one will observe where the force falls when we handle or do any thing with it, he will find it to be on the muscles of the left side of body, and more particularly on those of the loins and long extensors of the back."

"If the greater forces then exerted by our right arm fall particularly upon the muscles of the left side of the spine, so likewise will the lesser ones; consequently, it is the muscles of that side which we find most frequently to suffer disorganization, and become contracted, owing to the cause of this disorganization being greater in them than in those of the opposite side, and is the reason why contortion is so often to the right side.

"The muscles then of the left side of the spine being, from the causes above mentioned, those which suffer disorganization in particular, and more especially the *quadratus lumborum*, *sacro-lumbalis*, and *longissimus dorsi*, they, by the force of their diseased contraction overcoming the action of the muscles of the right side, rotate the vertebræ to which they are attached, because of the angles formed relatively between the vertebræ and the pelvis (the points of origin and insertion of these muscles,) and the force of their contraction acting upon moveable horizontal or transverse levers, namely, the transverse processes of the vertebræ."

From these principles, which the author conceives to be unquestionable, he concludes, that though the rotatory motion of each vertebra is limited and trifling, their multiplied effect is considerable; and so twists or contorts the whole column, as to give its natural flexures a lateral turn, and thus make it be thought to have the same number of curvatures.

Such may be said to be the mode in which Dr. Dods accounts for lateral curvature of the vertebral column; or, in other words, the theory of Dr. Dods is, that the column, instead of being allowed to follow its usual, if not its natural direction and curvatures, is compelled by the inordinate action of certain muscles, to make these bends or curvatures laterally. Perhaps our readers may feel some interest in knowing by what course of observation, or by what process of reasoning, Dr. Dods was led to adopt a theory, in which, it must be granted, there is something plausible and specious. As we are unwilling to allow his arguments to suffer by our mode of statement, the author shall speak in his own person.

"Having been induced, of late years, to apply myself

more particularly to the investigation of this interesting deformity, and being previously convinced that it depended more upon muscular contraction, brought on by the fashionable modes and customs of the day, than upon structural disease of the vertebræ, I was led to the adoption of friction for its cure : and in order that I might give it the fairest trial possible, I was induced to become the operator myself.

" During the course of my operations upon several patients, I was struck, in all of them who were contorted to the right side, with a considerable bony hardness and projection on the left side of the loins, raised nearly to a level with the spinous processes ; and this I found to be the case in the patients whose spine exhibited little or no apparent curvature in the loins, as well as in those in whom the apparent curvature was very great. Being led to investigate this anomaly, I re-redoubled my exertions to discover the cause ; and I found, after the muscles had been relaxed by friction (for they were in every case extremely rigid,) that the bony projection was the transverse processes of the vertebræ of the loins, which I could now as distinctly feel and count as the spinous. From this circumstance, I was led to examine whether I could feel the transverse processes of the same vertebræ on the opposite side ; but without effect, for they appeared to have sunk inwards, completely out of reach. Having satisfied myself of these facts, I then reasoned in the following manner.

" If these distortions of the spine, as it is generally supposed, depended upon a direct lateral curvature of the column, the transverse processes, although they would be, in this case, separated from each other on the one side, and approximated on the other, yet would they not be altered in their transversity, with respect to the body, and consequently ought to be as easily discovered and felt on the right side as on the left, but which I found was not the case. I then asked myself what movement of the vertebræ would bring their transverse processes to be so prominent, and so distinctly felt on the one side, while they were totally out of reach on the other, and I concluded it must be their ROTATION.

" Being satisfied that this movement had taken place in the vertebræ of the loins, I suspected that the same had occurred to those of the back ; but as I had no transverse processes here to guide me, similar to what I had in the loins, because of their attachment to the ribs, I was obliged to confine my examination to the spinous processes, which I found evidently to lie obliquely over to the right side, in contact nearly with the ribs, which were raised up in an opposite direction.

" Being now convinced, also, that the vertebræ of the back were rotated like those of the loins, I concluded that those of the neck had undergone a similar change ; and, therefore, that the whole column was spirally twisted, and exhibited to me the profile of its natural flexures.

" Although satisfied in my own mind, from my examinations, that such a movement of the whole vertebræ was possible, and had actually taken place, yet I found that a more certain proof of it was needed. I therefore, as the best mode of affording this, had recourse to the rotatory motion of the head ; reasoning thus :—

" If the whole spinal column be spirally twisted by the aggregate rotatory movement of the vertebræ, the rotation of the head on its axis will prove the fact. For, as the head is equally limited in its rotation to both sides of the body, and as the spinal column is the pivot on which it turns, so will it be increased to the one side, and diminished to the other, relative to the trunk of the body, exactly in proportion to the extent of rotation of the column, if such shall exist.

" My reasoning in this case, I found, was correct: for, on making my patients, in whom the whole vertebræ of the column had moved round, rotate the head while in the erect position, I found that they could carry it, in this motion, considerably more to the left side than to the right, which was exactly what I conceived would happen. To satisfy myself still farther, as to the reality of this circumstance, I caused one of my patients, whose spine I considered to be more rotated than the rest, to lie down upon her back on the floor, and in this position to try the same experiment, and I found that she could touch the rug on which she lay quite easily with the left cheek, while she could not do so with the right, by more than three inches. This experiment I did not consider so certainly conclusive as the first ; for I thought the projection of the right ribs behind might have some share in causing such a difference, although I had no reason to suppose that it could be influenced much by it. The same experiment I made with my other patients, whose spines were similarly rotated, and the result in all of them was the same, but in a lesser degree, for neither of them were so much twisted as the one before mentioned ; which completely convinced me that the complaint is not CURVATURE, but ROTATION *alias* CONTORTION.—

Now, that it cannot be denied that muscular action, more particularly if it be inordinate or irregular, changes the position and modifies the figure of bones, we are of opinion that

many more conditions are requisite than Dr. Dods has admitted, to establish the theory which he has here delivered. Distortion cannot be produced without muscular action, but other causes are requisite at the same time to accomplish the effect; and as Mr. Shaw has stated 'it cannot alone be the cause of common lateral curvature of the spine.'

"The principal muscles," continues Mr. Shaw, "which support the spine in its natural position, are in two great masses, which pass from the pelvis to the head. Although these have been divided, in an arbitrary manner, by anotomists, into many distinct muscles, still the several divisions are admitted to have nearly the same action, viz. that of supporting and giving motion to the spinal column. But, to avoid the objections that may be made to arguments founded on considering the muscles on each side of the spine as forming only one great muscle, I shall confine my observations to the condition of the *sacro lumbalis* and *longissimus dorsi*, in the common cases of distortion.

"As the part of the spine between the origins and insertions of these muscles, is almost invariably curved in two opposite directions, before we can agree in the opinion that lateral distortion depends on the irregular action of the muscles, we must believe that the lower portion of the muscle on the one side, may be in forcible and unnatural action, while its upper part is relaxed; and that in the muscle of the other side, the reverse takes place. But this cannot be admitted; and indeed the more the theory is inquired into, the more erroneous it will be found to be. If the distortion depended on an undue action of the muscles of one side of the spine over those of the other, the curve would be always in the form of a single arch, instead of its being of a serpentine shape, as it generally is, between the points from which the muscles arise and those into which they are inserted. It has been stated, by way of argument in favour of the opinion, that the muscles on the concave side of the curve are stronger than those of the other; but I suspect that this statement is not founded on proofs derived from an examination of the muscles in the dead body, for I have found those on the convex side to be the largest. Indeed, there is one circumstance in the condition of the nerves of a distorted spine which would induce us to believe that the muscles on the concave side would be even more deficient in energy than those on the convex side; for the nerves which pass to them, are diminished to less than one half their natural size."

Dr. Dods appears entirely to overlook the circumstance,

that his theory supposes the muscles of one side to be greatly stronger than those of the other; yet he neither proves that this is the fact, nor even advances any arguments to show that it must be so. The disorganization, of which he speaks, if it actually was the consequence of the system of training and bandaging so feelingly described by him, must have been nearly uniform in every muscle of the parts subjected to unnatural constriction. Yet it is certain that this disorganization is not so extensive ; and we must be permitted to doubt, whether any disorganization of the kind could take place without effects much more immediate, much more distinct, and much more formidable, than those which are generally seen after a short training to the habits and attitudes of a young-ladies' boarding-school.

But even these objections, which must be obvious to many, are not the strongest to the theory now advanced. Dr. Dods seems to go entirely on the supposition, that his disease of contortion or rotation, is confined entirely to young ladies or delicate boys, whose persons are moulded to the fashion of the boarding-school machinery. Now, this might be true enough, providing lateral curvature occurred in no other persons than in those thus reared. But it is well knowu that this deformity takes place in many persons ere they have been subjected to this system of restraint, in not a few who never knew what it was, and that it was incident to the human spine before such mechanical means were contrived to counteract it. These objections are convincing proofs that the muscles, or their irregular action, are not the sole cause of lateral distortion, and are, as in all other cases of distorted bones, merely accessary to some peculiar condition of the bones, which allows them to be twisted in an unnatural direction. We will not deny, however, that Dr. Dods may be right in the view which he has taken of one or two instances which have fallen under his notice, and that his *muscular* hypothesis will explain such slight cases as those of which he speaks. His whole train of reasoning is indeed moulded in the most cautious and circumspect manner, and thus presents an air of plausibility which makes it appear to be established on solid grounds. Our readers may perceive, that he goes entirely on the principle of the spine being twisted, or its individual bones slightly turned round only ; and not bent or incurvated, as others generally think. It is not contortion, or curvature, but *rotation*, that constitutes the deformity of which Dr. Dods speaks; and to this he makes his hypothesis correspond with considerable dexterity, and some accuracy. But the corres-

pondence extends no further. The hypothesis of our author will no more explain the cases of distortion represented and described by Mr. Shaw, than it will account for the growth of giants, or the eruptions of Mount Etna. The agent to which Dr. Dods ascribes such effects, is unquestionably powerful; but neither of itself, nor in the manner described by him, is it adequate to produce all the varieties of distorted vertebræ which are presented to daily observation. The hypothesis is in short of partial application only.

2. The second opinion, the merit of which is examined by Mr. Shaw, is that which ascribes lateral curvature to disease of the ligaments. We know not whether those who maintain this opinion are aware that it was originally proposed by Ambrose Pare, to account for all spinal deformities. "Now that ligament wherewith we said the joints of the *vertebræ* were mutually knit and fastened, is encompassed with a tough and glutinous humour for the freer motion of the vertebræ. Sometimes another cold, crude, gross and viscid humour, confused and mixed herewith by great defluxions and catarrhs, begets a tumour, which doth not only distend the nerves proceeding forth of the holes of the *vertebræ*, but also distends the ligaments wherewith they are bound together: which so distended, and, as it were, drawn aside, do draw together with them the vertebræ, one while towards the right side, another while to the left, sometimes inwards, otherwhiles outwards, and thus move them out of their seats and dislocate them. A dislocated *vertebra* standing forth and making a bunch, is termed in Greek *Cyphosis;* those thus affected we may call Bunch-backed. But when it is depressed, it is named *Lordosis;* such we may name *Saddle-backed;* but when the same is hexated to the right or left side, it makes a *Scoliosis* or crookedness, which wresting the spine, draws it into the similitude of this letter *S.*"

A modification of the same opinion is given, first by Henry Bacheracht, in his inaugural dissertation published at London in 1750, afterwards by Pott, as we have before stated, and more recently by Monteggia, whose work we have already had occason to mention with respect. The opinion of Pott is expressed in the following terms. "When the ligaments and cartilages of the spine become the seat of the disorder, without any affection of the vertebræ, it sometimes happens that the whole spine, from the lowest vertebræ of the neck downwards, gives way laterally, forming sometimes one great curve to one side, and sometimes a more irregular figure, producing general crookedness and deformity of the whole trunk of the body, attended with many marks of ill health."

On this passage we have only to remark, that Pott would not have expressed himself so confidently, without sufficient evidence for the truth of his opinion. "In the skeletons of the hump-back (gobbi,") says the Italian author, "the intervertebral cartilages, and the bodies of the vertebræ, are observed to be compressed and diminished at the angle of the bend; —which crushing may be as much the cause as the effect of the morbid incurvation. Let the height of the bodies in the cartilages of several vertebræ be supposed by any accident to be diminished, this will cause a greater approximation of the pieces composing the spine, an incurvation before and of the sides, according to the seat of the parts crushed together. But this same compression may be a result of the weakness of the muscles, and posterior or lateral ligaments of the spine, by which it is made to fall forwards or laterally; and, in consequence of this fall, the bodies of the intermediate *vertebræ*, and the respective cartilages, will be found unusually compressed and trodden." According to this view, the author explains not only lateral, but antero-posterior incurvation in some cases by an affection of the intervertebral cartilages, the lateral and posterior ligaments, and even the bodies of the vertebræ. While the author is inclined to regard the changes from which he draws these inferences as the cause of incurvation, it is to be regretted that he had not taken some means of ascertaining, whether they were not effects of another cause.

Every opinion has its advocate; and the advocate of this is Dr. Harrison, who some time ago ascribed all the changes in the direction of the vertebral column, to relaxation and preternatural elongation of the lateral and posterior ligaments, and consequent displacement of the vertebræ in the usual motions of the trunk and spine. We have not space to enter into a minute inquiry into the grounds of this doctrine; but we may be permitted to say, that its author has adduced no authentic or unequivocal facts in its support; and it consequently rests entirely on the force of his own belief and judgment. It is obvious, that to search for means of refuting such a doctrine, would be idle indeed; and we therefore conclude what we intend to say on this head, by the following remarks from Mr. Shaw.

"In the spines of those who have been much distorted, I have found the intervertebral substance diminished on the concave part of the curve, in the same proportion as the bodies of the vertebræ, but I have never found any mark of disease in it. On the contrary, in the cases which I have examined,

it has retained all its peculiar firmness and elasticity. I may particularly refer to the specimens numbered 12 and 13 in the Appendix; for as they are not dried, but preserved in spirits, they still afford proofs, not only of the invertebral substances having retained all their characteristic qualities, but also of all the other ligaments being in a healthy condition. This is especially the state of the anterior and posterior ligaments of the bodies, and of the elastic yellow ligament at the roots of the spinous processes.

"When the ligaments are diseased, distortion of the spine is produced, but of a very different character from that which forms the subject of the present inquiry. Some instances are given in the Appendix; but the subject is so important, that I reserve the full consideration of it to the next volume."

3. The next opinion, whether distortion depends in any case on dislocation, involves a question of some consequence, and which has not yet been determined in such a manner as to furnish any positive conclusion. Mr. Shaw agrees with most of the best authorities in regarding dislocation of the vertebræ as either impossible or very rare, without fracture; and is of opinion, that no authentic case of distortion from this cause has yet been adduced. He is then led into a long examination of certain cases adduced by Dr. Harrison, in the Medical and Physical Journal, vol. 44th, as examples of distortion in consequence of dislocation. This involves part of the previous question of relaxation of the ligaments, and part of another inquiry,—how far external violence may operate in producing distortion. The discussion, which is somewhat controversial, would occupy too much of our readers' time to enter into it fully, and we must not pretend to take a partial view of it only. As we must leave something of Mr. Shaw's work to the discretion of our readers, we shall therefore not scruple to make this one of those subjects, on which they may peruse his observations at their leisure. The general result is, that according to our author's researches, lateral curvature is never the consequence of dislocation of the vertebræ.

Mr. Shaw having thus settled these several subjects of pathological investigation, proceeds in the next section to inquire into another opinion,—namely, whether lateral distortion is caused by disease of the vertebræ. We do not perceive that he expresses himself either with perspicuity or precision on this point; or at least he leaves it almost in the same uncertainty in which he found it. He begins, by stating, that as most eminent surgeons entertain this opinion, it is safer to

proceed on it in practice ; but questions its justice, as it not only is the cause of a patient being subjected unnecessarily to severe treatment, but in some instances of an increase of the distortion. He does not deny that disease of the spine will produce dislocation ; but he states his conviction, that many instances of distortion said to succeed this cause, will be found to be very different from that denominated *lateral curvature*, being either occasioned by vertebral ulceration, or by rickets. He then adverts to those reports of the state of the vertebræ in persons who have died affected with lateral curvature, in which it is stated that the bodies are softened, and full of scrofulous substance. This statement, he contends, is incorrect, and in the greater number of instances he affirms the internal structure of the vertebræ to be natural. As the explanation given by Mr. Shaw deserves to be known to those who examine bones said to be diseased, we give it in his own words.

" If the vertebræ of a patient, who has long been confined to bed, be examined, the appearance described above is found ; but if the person has been in the habit of taking exercise a short time previous to death, the bodies of the vertebræ are discovered to be as firm and compact as those in a perfect spine. I trust that it is now almost unnecessary to add, that instead of admitting that the softened condition of the vertebræ affords a proof of the existence of a scrofulous disease, that it should be considered as merely showing the bad consequences of confinement and want of use. The wasted appearance of the muscles, which is occasionally observed in cases of lateral curvature, and which has been confounded with their state during the inflammatory stage of the carious diseases of the spine, also depends on the same causes as the softening of the bones.

" It is well known that the shape of the vertebræ is materially altered in cases where the spine is much distorted ; but as no mark of disease is discovered when a section of the bones so mis-shapen is made, we may infer that the change of form is a consequence that may be produced independent of any specific disease existing in the bones, especially as it is found to correspond to the direction in which the pressure has been made.

" In the sections of the spine, marked in the Appendix 11, 12, and 13, although the bodies of the vertebræ are much altered in shape, their internal structure is not in the slightest degree different from that of vertebræ in a natural state ; there are scarcely even any marks of anchylosis on them, while some of the transverse and articulatory processes, and

even the ribs, are not merely anchylosed, but studded with exostosis. As similar results have been observed in many other instances, they may be considered as additional proofs that the bodies of the vertebræ, in cases of lateral curvature, are seldom affected with a specific disease, and are not even so liable as the more solid bones to be inflamed by the irritation caused by pressure."

He afterwards admits that these observations do not accord with the complaint which is generally made, of pain in the back; but ascribes this in slight cases to weakness, or to changes in the position of the bones.

To what cause, then, it may be asked, is this horrible and unmanageable deformity to be ascribed? Mr. Shaw has destroyed even the last resource of explanation; and, no more than ourselves, agrees with any of the former hypotheses. If it is neither the result of vertebral disease, nor of disease of the ligaments and fibro-cartilages, nor of rickets, nor of *mollities ossium*, whence is it derived? Mr. Shaw himself does not assign any positive reason in exchange for those of which he deprives the profession. In the subsequent section, which is a further illustration of the varieties of distortion, we find indeed some more unequivocal information. When speaking of the serpentine form of spine represented in figure second of the first plate, he makes the following remarks: "Although the necessity of attending to the effects produced by this serpentine form of the spine must be obvious, there are other practical questions illustrated by the preparation represented here. The lower dorsal and the upper lumbar vertebræ are not only much altered in form, but are also partially anchylosed; and even a considerable quantity of exostosis is thrown out on their sides. Two circumstances most important to be recollected in the treatment of distortions, are proved by this condition of the spine; first, *that a considerable degree of inflammation occasionally exists in the vertebræ in such cases;* and, secondly, that it is sometimes impossible to cure even a slight distortion of the spine."

Is the "considerable degree of inflammation" mentioned here a cause of the distortion, or its effect? We could have wished that Mr. Shaw had mentioned the marks or signs by which he inferred this inflammation; whether the vertebræ were enlarged or prominent, soft or hard, corroded by ulceration, or of an extraordinary brittleness; and we could have wished him also to have stated such proofs, as might have shown these marks to have preceded, to have followed, or to have accompanied the process of distortion. He afterwards

speaks of anchylosis, and the obscurity of its pathology; but it is not easy to see in what manner Mr. Shaw can employ this change in illustration of a deformity, which he contends, is quite independent of any morbid state of the bones. The arrangement of this part of his Treatise is much embarrassed with confusion and disorder. In this penury of specific information, we resumed the perusal of our author's Preliminary Observations on the Pathology of Distortion, in hopes to find something tangible on the organic changes which terminated in distortion. Here, however, our success was not more gratifying than in other parts of the work: and we were compelled to surrender our credulity to sundry speculations on the effects of action and inaction on the structure and consistence of such animal substances, as bone, cartilage, muscle, cellular membrane, &c. By several observations on the influence of the same agent on the consistence of bone, we are led to believe, that Mr. Shaw regards mere inaction, by softening the bones, to dispose to distortion; and, in the course of a few pages more, he informs us, that this law is no where better exemplified than in the history of the Affections of the Spine.

"The muscles, whose office it is to support the vertebræ, may be so weakened by want of exercise, as to become incapable of performing their functions. When this takes place, the vertebræ and the ligaments which bind them together yield to the superincumbent weight, for they are affected in a secondary manner by the same causes that have produced debility in the muscles. Weakness of the muscles is, therefore, perhaps, one of the most frequent forerunners of distortion; though there are many other causes which I shall have occasion to point out, in describing the various kinds of deformities."

It is really much to be regretted, that a surgeon, whose opportunities of acquiring information in pathological anatomy are so great, should be so far from communicating it either in a specific way, or in an intelligible form.

We pass over several intermediate pages on the changes induced on the form of the chest, either with or without curvature of the spine, to notice three concluding aphorisms with which the author introduces a section at page 124. These aphorisms are, first, "That the spine may be distorted to the greatest possible degree, although the bones are not affected with rickets; secondly, That in such cases the pelvis is seldom or never affected; and, thirdly, That the pelvis is never distorted by the application of instruments, in cases where the distortion is independent of rickets, or *mollities ossium*." On the first of these aphorisms, the author does not pretend to

enter into a minute or argumentative discussion; but merely refers to various instances of curved spine, and especially to one which he has delineated in the fourth plate, and in which the dorsal and lumbar vertebræ are undoubtedly distorted out of all form, while the bones of the pelvis are perfectly symmetrical. He informs us, however, that the proofs on which this opinion rests, were first drawn from comparing a number of distorted skeletons."

"From these facts, I drew two conclusions, which, if correct, will be acknowledged to be of some importance in practice. 1st. That unless some of the long bones, or the bones of the head, be affected, however distorted the spine may be, we are not to consider the disease as rickets, *i. e.* that the distortion does not depend on a specific disease of the bones; and, 2d, That the spongy bones (as of the spine, carpus and tarsus,) are not so liable to be affected by rickets as the long bones, and those which are more compact in their structure."

On this we have only to remark, that it is obvious that Mr. Shaw, in forming the aphorism now stated, proceeds all along on the principle, that rickets is a disease of the whole skeleton, and that no bone is exempt from its perversion. We admit that this doctrine has been generally taught by instructors, and admitted by disciples; but we are not aware that it rests on any better foundation than mere assumption. It is certainly neither inconsistent with fact, nor quite absurd, to suppose a certain number or set of bones only affected by rickets; nor is it impossible to suppose different parts of the skeleton to be affected in different degrees. Unless, therefore, Mr. Shaw can establish this doctrine on a more substantial foundation than that on which it now stands, we do not perceive, that the inference regarding the independence of spinal distortion and the rachitic *diathesis*, as it may be termed, is of any force whatever. The whole proposition in truth rests on the opinion which Mr. Shaw entertains of the nature of rickets, whether he regards it as in every instance a constitutional disease, or, as we are convinced it is in some instances, an affection merely local. Viewed in this light, the aphorism of our author is neither true nor false; but simply expresses, in language indeed somewhat different, this fact,—"that the spine may be distorted to the greatest possible degree, although the other bones of the skeleton are not distorted."

Having settled the first proposition in this manner, the others which immediately depend on it, are easily disposed of. In this view indeed, the instances of symmetrical and well-

formed sculls, and *pelves*, such as those recorded by Sandefort in persons with crooked spines, those in Mr. Bell's Museum, in the Museum of the College of Surgeons, and in Dr Hunter's Collection at Glasgow, are mere proofs of the local nature of rickets, which we have already ventured to admit.

We hasten to the practical measures investigated and proposed by our authors.

Both are strongly in favour of friction, in whatever mode it is practised. Mr. Shaw strongly reprobates the attempt to reduce vertebræ supposed to be displaced; and, indeed, no one acquainted with the structure of the vertebral column, will ever propose a measure so unreasonable, or so likely to be injurious. Rest on the inclined plane for several hours daily, has been much recommended by many practitioners of late years. The arguments offered in its favour were founded on the idea that the distortion depends on undue contraction of the muscles of the spine, and on a diseased state of the vertebræ. Mr. Shaw disapproves of it in the treatment of lateral curvature; because the disease proceeds originally from weakness, and the body is weakened instead of being strengthened by such management. The plan which Dr. Dods proposes is completely at variance with this principle. By his own account he actually places patients on their back on a concave elastic couch, on which there is a mattrass two or two and a half-inches thick;—a measure which is likely to increase rather than diminish the deformity. It ought to be remembered, that nothing is a remedy unless when seasonably and judiciously applied: and this is to be said of the horizontal position and the inclined plane. There is little doubt that, occasionally used, it is a very convenient and effectual mode of obviating distortion in young persons of tall, slender figure and delicate frame. To confine an individual to it for months, unless there is positive disease of the vertebræ, which may be generally ascertained by care and attention, is evidently useless, and must defeat the purpose intended.

The use of machines, and mechanical contrivances in general, both Dr. Dods and Mr. Shaw agree in condemning. Dr. Dods thinks they cause, rather than prevent deformity; and Mr. Shaw has adduced several instances of distortion, which, instead of being cured, were aggravated during the employment of these instruments. What has been already said of the horizontal position, may, however, be justly applied to mechanical restraint;—that there are kinds of distortion which may be prevented from getting worse, and that, in some instances, their use is admissible under proper restric-

tions, and with adequate superintendence. The opinion that they ever cure, is now almost entirely abandoned.

On the subjects of Exercise, and the other details of treatment, we refer to the works themselves.

MONTHLY SUMMARY

OF PRACTICAL MEDICINE.

I. ANATOMY AND PHYSIOLOGY.

Dr. Wallaston *on Single Vision with Two Eyes.*

So long as our consideration of the functions of a pair of eyes is confined to the performance of healthy eyes in common vision, when we remark that only one impression is made upon the mind, though two images are formed at the same moment on corresponding parts of our two eyes, we may rest satisfied in ascribing the apparent unity of the impression to habitual sympathy of the parts without endeavouring to trace further the origin of that sympathy, or the reason why, in infancy, the eyes ever assume one certain direction of correspondence, in preference to squinting.

But, when we regard sympathy as arising from structure, and dependant on connexion of nervous fibres, we therein see a distinct origin of that habit, and have presented to us a manifest cause why infants first begin to give the corresponding direction to their eyes, and we clearly gain a step in the solution, if not a full explanation, of the long agitated question of single vision with two eyes.—*Philosophical Transactions.*

M. Savart *on the Internal parts of the Ear.*

M. Savart has published the results of numerous experiments, made with a view of ascertaining the uses of the membrana tympani and internal parts of the ear. Referring to the paper itself for the details, we must content ourselves with enumerating the conclusions at which he has arrived. They are—1st. That the communication of vibrations by means of air appears to observe the same laws, at least with respect to

minute distances, as those which govern solids. 2d. It is not necessary to suppose, as has been done heretofore, the existence of a particular mechanism to make the membrana tympani always vibrate in unison with the bodies which act upon it. 3d. That the degree of tension does not vary, except to augment or diminish the amplitude of its excursions (*excursions*,) as supposed by Bichat; but with this difference, that M. Savart's experiments prove the reverse of Bichat's conjecture, which was, that the membrane was relaxed by powerful impressions, and became tense from those which were feeble. 4th. That the vibrations of the membrane are communicated without alteration to the labyrinth, by the medium of the ossicula, as the vibrations of the upper table of a musical instrument are transmitted to the lower. 5th. That the ossicula likewise modify the extent of movement in the vibrating parts contained in the labyrinth. The author further thinks that the extent of the membrane of the tympanum in different animals influences the number of sounds which they can perceive, and likewise the limits at which these sounds commence, or cease to be perceptible to the animal.—*London Medical and Physical Journal.*

II. SURGERY AND MIDWIFERY.

Mr. Yeatman *on the Treatment of Hernia.*

In army and private practice, twenty-four cases of strangulated hernia have been treated by me with success; twenty-one of which were reduced by using the following means, and three were operated upon at an early period of the strangulation. Fifteen of the above were cured by venesection, the application of cold, and the taxis; and six by these means, added to the internal exhibition of five grains of opium. The intestine, in all the twenty-four cases, had been strangulated from one to five hours. Each patient was immediately bled *ad deliquium;* then placed on the floor of the room, on a blanket, with pillows to raise the pelvis, the thighs being bent, and the taxis managed in the usual way. Pails of cold water, with ice, or muriate of soda and ammonia, dissolved in the water, were placed by the side of the patient, and poured on the hernial tumour, at the distance of two feet, from the spout of a large tea-pot or small tea-kettle, for a space of time varying from an hour to an hour and a half; occasionally employing the taxis for a few minutes together. When this mode of treatment failed of success, five grains of opium were given.

in each case, the patient put to bed, and the taxis had recourse to during a profound sleep.

The above number includes all the cases of strangulated hernia which have fallen under my care, or for which I have been consulted; and the above are the only means which have been employed or recommended by me.

It is well known that cold, by condensing the rarefied air in the intestine, diminishes the volume; and that a large dose of opium produces muscular relaxation, particularly if the patient be bled largely prior to that drug being swallowed. I pass on, therefore, to observe, that, to use the lancet, cold, and opium, with far more success than common, they must, at least, be employed in the above manner, and to the above extent.

Concerning the operation for hernia, it may here be said, that it is most disgraceful to the surgeon to find the contents of the hernial sac in a state almost approaching to gangrene, when he might have operated at an *early* period of the strangulation: but, alas! all the remedies which have ever been recommended in such cases, are put in requisition, and many hours, and even days, are too frequently suffered to elapse in imbecile and reckless attempts to return the inflamed, enlarged, and strictured parts into the abdominal cavity.

It is now seventeen years since I had occasion to observe, before the Westminster Medical Society, that the surgeons of the Bristol Infirmary, (my uncle having been one of them, and myself a pupil at that excellent hospital,) usually employed, with the taxis, only three or four of the more powerful remedies to reduce the hernial tumor,—as venesection, cold, and tobacco; and that, if they did not succeed in the course of three or four hours, they proceeded to operate: and, ascribing the cause of their great success to the circumstance of operating at an early period of the strangulation, I well recollect that that eminent surgeon, Mr. Brodie, coincided with me in opinion, and both ably and warmly supported it.—*London Medical and Physical Journal.*

Mr. Averill's *Case of Calculus in the Bladder.*

Thomas Bennet, aged nine years, of Ripple, Worcestershire, had had symptoms of stone in the bladder ever since he was a year old. About the middle of April, 1824, I by chance saw him, but having no sound with me, did not examine him.

On the 17th of May he came to Cheltenham, when I sounded him, and felt a stone. On the 19th I sounded him again, in the presence of several other surgeons, who all felt the stone distinctly. It was then determined that the operation of lithotomy was the only means of giving him relief, and he was accordingly directed to live abstemiously, that is, to subsist on vegetable diet, and take mild aperient medicine. He had for some time discharged a considerable quantity of yellow matter with his urine, which always sunk and adhered to the bottom of the vessel. He, at times for the last two or three years, had been in the most excruciating agony, and always after being sounded. The medicine which he took for the fortnight prior to the operation, was at the first small doses of castor oil, with mucilage of gum-arabic, and tincture of henbane; and afterwards small doses of emetic tartar, with Epsom salts.

On the 31st of May, at 12 o'clock, in the presence of most of the medical men of this place, I performed the operation, some thin gruel having been injected into the rectum, and returned about three hours previous, and the following draught given at 11 in the morning.

℞ *Tinct. opii gtt. xxv. Tinct. hyoscyami gtt. xxx. Mucil. acaciæ ʒii. Mist. camph. ʒi M. ft. haust.*

The operation was performed in the following manner, Mr. Key having very kindly lent me his case of instruments. The extremities being secured, a straight staff was passed into the bladder, when an incision was made in the usual direction, till the groove was exposed. The same knife was carried along the groove, in the manner directed by Mr. K., and the prostate gland divided throughout its whole extent; then, directing my nail along the groove in the staff, I felt my way into the bladder, till the point of my finger came against the rough surface of the stone, when I withdrew the staff, and having used my finger as a conductor for the forceps, I withdrew it also. I then took hold of the stone, and endeavoured to extract it, but in vain. The division of the prostate had not left an opening of sufficient dimensions to allow the stone to pass, and the forceps slipped from the calculus. I was therefore under the necessity of enlarging the opening into the bladder. This I did, by passing into its cavity my left index finger, which I used as a director for Sir Astley Cooper's beaked knife. With this instrument I further dilated the cut, to the extent, as I thought, of about a quarter of an inch. On again introducing the forceps, I was some time before I could get hold of the calculus; but having once seized it, I drew it from the bladder. It was

of the mulberry kind, and the largest I have known for a boy nine years old, weighing one ounce and a quarter avoirdupois. The operation lasted about seven minutes; but little blood was lost. The patient was put to bed, ordered to be kept quiet, and directed to drink freely of the following mixture.

℞ *Sodæ subcarbon.* ʒii. *Mucil. acaciæ* ʒiii. *Decoct. hordei* ʒxxi. *M.*

For some hours he dozed occasionally, and complained of a good deal of smarting in the wound; but in the evening, at 8 o'clock, he was free from pain and comfortable.

In the afternoon of the following day, slight symptoms of peritoneal inflammation came on, for which nine leeches were applied to the lower part of the abdomen, and a purgative medicine ordered. These means removed the bad symptoms, and he continued improving daily. A little urine passed occasionally by the urethra, but by far the greater quantity through the wound *in perinæo*. On the morning of the 8th of June, I was sent for to see him, and found him in great pain, in consequence of the wound in the bladder having closed, and allowed that viscus to become distended with urine, which he was unable to pass off by the urethra. He was put in a warm bath, but derived no benefit from it. A small catheter was, therefore, passed by the urethra. It entered the bladder readily, when about half a pint of urine passed off, which relieved him. The urine was drawn off again in the evening. On the 9th the bladder was again distended; the catheter was therefore introduced, and the urine drawn off. This day the urine passed again by the wound *in perinæo*, and continued to do so on the 10th. On the 11th the bladder was again distended, without the power of evacuating itself. A catheter was again introduced, when the urine passed away. In the evening the urine had collected again, when, on slight pressure being made above the pubes, the bladder contracted and emptied itself without further assistance. From this period there was no necessity for the catheter, the bladder acting without assistance, and the whole of the urine passing through the natural channel. On the 14th, or just a fortnight from the operation, he left his bed for an hour or two. The following morning he was dressed, and sat up greater part of the day. On the 17th the wound was completely healed, and he walked a little in the open air. On the 24th he left Cheltenham quite recovered, the discharge of matter with his urine having gradually diminished since the operation, and he having perfect control over his bladder.

Dr. Sherlock Willis of this place attended the case with me, throughout its progress.

The above will, I trust, serve as far as one case can, to make Mr. Key's method of operating more generally adopted. In the hands of those acquainted with the relative situation of the parts about the neck of the bladder, this will be found the most simple and safe operation. The straight staff I consider to be the great improvement; indeed it is the principal novelty in the operation, as Messrs. Lawrence, C. Bell, and S. Cooper, all occasionally cut into the bladder with the same knife with which they have opened the groove of the staff; but Mr. Key, I believe, is the first who has substituted a straight for a crooked director. Of the two latter instruments, the advantage which the straight possesses over the curved must be made obvious to any one who will take the trouble, by removing the *os innominatum* and lower extremity, to make a side view of the contents of the pelvis, and then use the two instruments separately as directors for the knife in cutting through the prostate into the bladder. In this way the eye can follow the knife throughout its course, when the safety (as a conductor) of the straight over the curved staff is strikingly exemplified. If, as in the case just narrated, the complete division of the prostrate with Mr. Key's knife should not leave an opening sufficiently large to allow the stone to pass, I should prefer, as I did, using a beaked knife to dilate the wound in the bladder, since, from its form and blunt point, it would be more readily passed along the finger, and be less likely to do mischief; besides the operator can feel more distinctly the extent to which he is cutting, in drawing the knife towards himself, than he can when he cuts in a contrary direction.—*London Medical and Physical Journal.*

III. PATHOLOGY AND THERAPEUTICS.

M. Andral *on Diseases of the Lymphatic System.*

The annals of science possess but few correct observations respecting the organic alterations of the thoracic canal, and of the vessels which terminate in it. These alterations are, however, very rare. I have dissected with care the thoracic canal in more than three hundred bodies, and I have only found it five times in a state of disease:—in two cases its parietes were the seat of an obvious state of inflammation; in a third case it was obliterated; in a fourth it was can-

cerous; and, finally, its parietes were, in the fifth case, sound, but a foreign, solid, and apparently a tuberculous, matter filled its cavity. The rare occurrence of such cases induces me to publish them: they furnish some materials to the history of diseases of the lymphatic system—diseases respecting which we possess as yet so few positive ideas.

I. *Inflammation of the Thoracic Canal.*—A female, forty-seven years of age, who was afflicted by chronic nephritis, died in the Hospital *La Charite* during the course of the year 1824. This female had arrived, by degrees, at the last stage of marasmus, and hectic fever had consumed her. She had also presented all the symptoms characterising nephritis. The right kidney was found changed into a number of cysts filled with pus: behind it existed a vast purulent collection, which extended as far as the iliac fossa. Instead of finding the thoracic canal empty, or filled with a little transparent serosity such as is generally found, I discovered, between the descending pectoral aorta and the vena azygos, a white chord, of the size of a writing pen. A more attentive dissection soon discovered to me that this chord was the thoracic canal distended by purulent matter. It was thus filled from where it passed through the diaphram to a little before it opened into the subclavian vein. In the whole of this extent, the internal surface of the vessel presented a red colour, the intensity of which was not the same throughout. In one part a number of vessels were seen agglomerated and admirably injected; and in another part the redness was uniform, and similar to what is frequently observed on the internal surface of arteries, but it seemed not to result from vascular injection; one might say that the internal surface of the canal, at these parts, was as if painted red. This colour resided entirely in the internal membrane, which was considerably thickened, and which could be separated from the external tunic much more easily than in a state of health. The rest of the lymphatic system presented nothing particular.

In this case were found united the most decided appearances of inflammation, viz. redness of the textures, their thickening, and, lastly, pus. But a difficulty may be here started—it may be said that the pus was not formed in the thoracic canal, but was carried there by absorption, and that it was in consequence of its contact with the internal surface of the canal that this latter was inflamed. In whatever manner the fact may be explained, it was not the less certain that there existed inflammation of this duct, either primitive or secondary.

I had occasion to see another case of inflammation of the thoracic duct at the Hospital for Children, in a boy eleven years of age, who had been under the care of Dr. Guersent, and who died of croup, complicated with pleuro-pneumony and gastritis, in the course of June 1823. The parietes of the thoracic canal appeared to me much thicker than natural, and also much more friable. The cavity of the canal contained but a small quantity of transparent serosity ; but its internal surface was, in its whole extent, of a lively red colour. Observed between the eye and the light, this redness seemed to arise from a minute vascular injection. A number of tumefied and reddened glands, with tubercular points in the centre of each, were developed along the whole course of the thoracic duct : a still greater number was found in the abdomen, around the enlargement in which the duct commences.

This case differs from the first, in the circumstance that no pus was observed in the canal. The existence of inflammation was, however, proved not only by the redness, but also by the change, as respects thickness and consistence, which the parietes of the canal had undergone. It should also be remarked, as a circumstance not observed in the first case, that inflammatory enlargement of the adjoining lymphatic glands was present in this case, along with inflammation of the canal.

II. *Obliteration of the Thoracic Duct.*—In the course of the month of November 1821, whilst opening; at *La Charite*, the body of a patient dead of pulmonary consumption, I proceeded to the dissection of the thoracic duct. In the extent of some inches above the diaphragm, this canal was filled by a very considerable quantity of lymph ; above this part it was abruptly constricted, and in the part corresponding to the bodies of the fifth, fourth, and third dorsal vertebræ, it was both constricted and deprived of its transparency ; finally, from above the third dorsal vertebra to its termination in the vein, the canal assumed its usual calibre and transparency, and was again filled with lymph. I opened the duct immediately above the diaphragm: a fine stilet easily passed until it reached the margin of the fifth dorsal vertebra ; but at this point, the stilet met with a sort of cul-de-sac, and could pass no higher—the canal was entirely obliterated in the part which was constricted, and transformed into a sort of fibrous chord. But how came the canal to be filled with lymph above the obliteration ? By what route had the lymph been carried into that situation ? A minute dissection soon enabled me to discover *the existence of a considerable lympha-*

'ic vessel—a sort of second thoracic duct—which arose from that part of the principal duct a little below the commencement of its obliteration, directed its course obliquely from below upwards, and from within outwards, until it reached the vena azygos, proceeded behind this vein, and afterwards reached the upper portion of the thoracic duct, and opened into it at the point above where the obliteration terminated, forming with it a sort of angle similar to that which the thoracic duct itself forms with the subclavian vein.

I do not attempt to explain how and when the obliteration now described took place. As to the collateral duct, by whose means the circulation of the lymph seems to have been preserved, it was chiefly remarkable with respect to its size. Indeed, it is not uncommon to find the thoracic canal accompanied in its course by more or less considerable branches, which are detached at various angles, and which, after a certain space, again approach it and open into it. It was probably, in this case, one of these branches, which, subsequently to the obliteration of the principal canal, acquired a much greater volume. This phenomenon is similar to the more familiar one which takes place when the canal of an important artery is obliterated. M. Rayer, in his excellent article on *Dropsy*, in the *Dictionnaire de Medecine*, has cited from Sir Astley Cooper a case of obliteration of the thoracic duct, in which, as in the one now detailed, the circulation of the lymph was preserved by means of collateral vessels, which were developed in proportion to the necessity.

III. *Cancer of the Thoracic Duct.*—A female died at 'La Charite' of cancer of the uterus, in the month of October, 1824. The neck of the uterus was observed, on dissection, to have been destroyed, and in its place a black tainted *putrilage*, a vagino-vesical fistula, and enormous cancerous tumours, were found in the pelvic cavity and in the mesentery. Scirrhous tumours of considerable size were developed before the vertebral column, from the superior extremity of the sacrum to the diaphragm. The thoracic duct, when laid bare in the chest, appeared as a white chord, and much more voluminous than natural. When it was opened, a whitish, puriform fluid escaped from it. A number of small bodies of a dull white, irregularly rounded, and the medium size of a pea, projected from its internal surface. These small bodies were continuous with the tissue of the parietes of the duct, and presented a perfect analogy of structure to the cancerous tumours developed in the abdomen. There were elsewhere no more tumours projecting into the duct, but its parietes were consid-

erably thickened ; and, as the thickening was not the same in all parts, the duct had an embossed appearance. Wherever this thickening of its parietes existed, it was observed to be owing to the development of a dull-white tissue, streaked here and there by reddish lines, and in other places already reduced to a pulp of a foul, reddish-grey colour, very analogous to the substance of a brain in which putrefaction has commenced (encephaloid tissue in its double state, of crudity and of softening.) Finally, near the diaphragm, the cancerous masses, which surrounded the thoracic duct, and which apparently had their points of origin in the lymphatic glands, were so considerable, that it was impossible to trace this vessel farther amongst them. The internal surface of the duct presented, besides, many bright red points; and this redness was more remarkable, the lower the part that was examined. It should be remarked, that the left subclavian vein, in which the duct opened freely, was distended greatly, from its origin to the cava superior and brachial veins, by clots of blood ; the most external layers of which had contracted intimate adhesions to the parietes of the vein, the internal surface of which was of a brownish and rugous red colour. The other veins presented nothing resembling this. Did there exist any relation between this condition of the subclavian vein and the cancerous state of the thoracic duct ?

IV. *Matter analogous to Tubercular Tissue, contained in the Thoracic Duct, and in several Lymphatic Vessels.*—A female died of cancer of the uterus, in 'La Charite,' in the course of August 1824. The glands of the mesentery, and those of the pelvic cavity, formed, anteriorly to the vertebral column, enormous cancerous masses. The glands of the groin, and those surrounding the bronchiæ before their entrance into the lungs, were, either wholly or in part, of a cancerous structure.

Lymphatic vessels, distended by a limpid and colourless serum, and presenting here and there white points, which were displaced by slight pressure, proceeded from several of the cancerous inguinal glands. The matter forming these white points seemed, therefore, only to be contained within the canal of the vessels : indeed, the vascular parietes having been divided, this matter escaped spontaneously, owing only to the elasticity of the vessels which contained them. The appearances of these white substances approached nearer to the tubercular than to the cancerous tissue. The lymphatic vessels, thus distended in places with this whitish matter, here rounded into small grains, there elongated into cylinders, could be ea-

sily traced under the crural arch, in the pelvic cavity, and to the middle of the cancerous mass in front of the spinal column. The thoracic canal disengaged itself from the midst of this mass, at the margin of the last dorsal vertebræ. In three or four places this canal was very much distended and obstructed, by a similar white matter to that which filled the lymphatics. This matter here formed masses, the largest of which equalled the size of a hazle-nut, and, as in the lymphatics, it was here contained in the cavity of the duct, without presenting any connexion with its parietes.

Finally, the external surface of both lungs was traversed by a number of whitish striæ, entirely resembling the disposition of the lymphatics when injected with mercury. These striæ were actually found to consist of lymphatics, filled with the same matter as that found in the other lymphatics and in the thoracic duct. Many of those were traced to the bronchial glands, which had degenerated into the cancerous state. The interior of the lungs, and particularly that of the left, also contained many of these vessels, resembling white threads, thickened at intervals. They were observed isolated in some places, and in others agglomerated in greater or less numbers, so as to form a species of plexus. The lungs contained no tubercles; and the parietes themselves, of the lymphatic vessels of the lungs, of the pelvic cavity, of the groin, and of the thoracic duct itself, presented no appreciable lesion.

The obstructed state of the glands, into which these lymphatics terminated, and the obstruction offered to the flow of the lymph from the thoracic duct, owing to the pressure of the enlarged glands in its vicinity, by preventing the course and the discharge of the lymph whilst the faculty of absorption continued, seem to us to have occasioned an accumulation of lymph in the vessels thus obstructed. The flow of the lymph being thus prevented, its serous portions may have been again absorbed, whilst its albuminous parts may have become accreted, or otherwise changed, by the vital influence of the vessels containing them, so as to present the same appearances as those possessed by the morbid accretions to which the term tubercular has been applied. It is evident that these accretions were not absorbed in this state; and the matter of which they were formed was essentially different from that developed in the uterus and glands; from which parts, indeed, the lymphatics containing these bodies did not *arise;* into the latter parts, certainly, these lymphatics *passed* or *terminated;* but this circumstance fully explains the view we have taken of

this very important (as far as the origin of tubercular formations is concerned) pathological observation.—*London Medical Repository.*

Dr. A. T. Thompson's *Case of Irritative Fever with Remarks.*

Whatever may be the cause, the fact is well ascertained, that more Medical Practitioners have suffered from accidents depending upon morbid dissections within these last ten years, than for half a century before the commencement of that period; yet, it is a remarkable circumstance, that, amongst the numbers who have recovered from these accidents, no individual has published his case for the benefit of the Profession.—Having lately almost lost my life from an accident of the abovementioned kind, I shall endeavour to supply the desideratum which I have noticed, by giving you a detailed account of my own case, with a few remarks on the nature and treatment of similar cases.

Early in the month of November last (1824,) I was requested to see a lady who had been labouring, for ten days, under a severe inflammatory affection. I found her in a state of extreme danger; and, indeed, so hopeless was the case, that I hesitated to use the lancet, on which alone, however, any prospect of benefit depended; for, although she had been previously bled, both topically with leeches and at the arm, yet, the excitement was such that nothing but further depletion promised even a shadow of success. She was, accordingly, bled to the amount of twenty ounces and purged freely; but she died on the following day. I may mention, that the inflammation under which she laboured, when my advice was demanded, was pleuritic; but, as her friends informed me that her head had been previously the seat of the disease, and it was only when the pain left the head that the pleura became affected, I concluded that the disease was Rheumatism, and that her dissolution was to be attributed to the metastasis which, sometimes, occurs in the acute form of that complaint. It may be proper to mention, also, that the arm in which she had been bled, previously to my seeing her, was painful, much swelled, and displayed all the symptoms of inflammation of the vein; to relieve which, it had been poulticed and kept cool with an evaporating lotion. She had been delirious during the night before the day in which I saw her; but, at the time of my visit, she was perfectly collected, and continued so until

the moment of her death, which was unattended by any struggle or convulsion. On the second morning, at half-past seven o'clock, thirty-two hours after her death, I opened the body.

The stomach, although it was greatly distended with flatus, yet, was perfectly healthy in appearance; as was the liver, the pancreas, the spleen, and, with the exception of one of the ovaria, every abdominal viscus. The uterus was unimpregnated, and the os tincæ completely pervious; the right ovarium was rather larger than usual; and a hydatid, the size of a pea, was appended to it by a kind of ligament. I particularly notice the unimpregnated state of the uterus, because an opinion prevails, that much of the danger arising from the wounds received in morbid dissections depends on something connected with the puerperal state. On opening the thorax, its left cavity was found to contain about a quart of bloody serum. The pleura costalis, from the third to the seventh rib, and from the spine to about the breadth of the hand distant from the sternum, bore evident marks of violent inflammatory action having recently existed, as did also the pleura pulmonum, corresponding to the above-mentioned space: both were covered with a reticulated web of coagulable lymph, but in no place did they adhere. The greater part of the principal lobe of the left division of the lungs was turgid with blood. There were no other appearances indicative of disease in any part of the body.

No accident occurred, either to myself or to the young gentleman who assisted me, in the dissection; but, in sewing up the body, having, improvidently, furnished ourselves with curved needles only, I received a slight scratch on the first joint of the fore-finger of the right hand, owing to the needle turning suddenly round whilst I was forcing it through the integuments of the body. The wound was so slight that I paid no attention to it at the time it occurred; nor was it sufficient to attract my notice until the evening, twelve hours afterwards, when it excited a slight degree of pain and appeared a little inflamed.—The pain, however, increased towards bed-time; awoke me after I had been asleep two hours only; kept me awake the rest of the night, and was accompanied with very profuse perspiration. In the morning the finger was considerably inflamed, and a small white spot appeared in the centre of the scratch, from which, on opening it with the point of a lancet, I squeezed a globule of pus. The finger was so much relieved after this slight operation, that I regarded the wound unworthy of farther notice; but, during my professional visits in the forenoon, I was attacked with rigors; my strength gradually fail-

ed, and my system became evidently under the influence of incipient fever. I, nevertheless, continued my visits, although I was, at length, scarcely able to ascend a stair; and, on returning home at two o'clock in the afternoon, I fainted as I was giving my orders to my assistant; but being laid upon a sofa, I soon revived, and became sensible of the nature of my alarming condition.

Although my mind was weakened by the state of my body, I was sufficiently collected to reflect, that as extreme prostration of strength was the most marked symptom of my attack, the best method of meeting it was to attempt to rouse the nervous energy, and, at the same time, to clear out the bowels: I therefore ordered for myself a mixture containing camphor, ammonia, and wine of colchicum; and had taken two doses of it before my friend Dr. Granville, who had been sent for on the occasion, arrived. I could not convey a more accurate idea of my sensations to the Doctor, than by comparing them to those which are said to result from the bite of a Cobra di capella, or other venomous serpent; or to those which I had once heard described by a person who had taken an overdose of Prussic acid. The debility, besides being excessive, was accompanied by such a feeling as I conceive must attend the approach of death, at the close of a disease of debility. The respiration was laborious, and accompanied by an acute pain under the xiphoid cartilage, extending to a short distance along the sternum; while the pulse was quick, vascillating and struggling, with occasional hard throbs, "which," to use Dr. Granville's words, "would have authorised bleeding in the hands of an inexperienced practitioner."

Dr. Granville concurred in the principle which had guided me in prescribing for myself, but disapproved of the colchicum; and, instead of the mixture which I was taking, ordered for me a bolus composed of three grains Camphor and four grains Cayenne pepper; and, as my extremities were cold, the entire surface of the body pale, and the features shrunk and cadaverous, he directed the feet to be bathed in hot water previous to my being conveyed to bed. The prostration of strength was, however, so great, that this could be effected only whilst I remained in the recumbent posture, in which position, also, my clothes were obliged to be taken off. The wounded finger was poulticed.

The bolus, which was repeated, and the pediluvium produced that reaction which was expected, and I obtained some sleep during the night. On the following day the pulse was upwards of 130, but small; the skin hot and dry; and there

was some degree of delirium; but these symptoms, as well as the pain under the xiphoid cartilage, which was still felt, although in an inferior degree, in the morning of this day, subsided as the bowels were freely opened, by a Calomel bolus and a brisk purgative, composed of a scruple of jalap and half a dram of supertartrate of potass, which Dr. Granville had prescribed. The pain of the finger, which was now swelled, stiff, and inflamed, was less severe than I anticipated; but it extended up the arm, and was slightly felt in the axilla. An evaporating lotion was substituted for the poultice; and three grains of James's powder were ordered this evening in combination with four of extract of henbane, instead of Cayenne pepper. I slept better this night; and, by continuing the use of the pills, and maintaining the full action of the bowels for the three following days, I was so much relieved as to be able to sit up for a few hours, and to take a little nourishment on the fifth day; and, on the sixth, I considered myself out of all danger. The pain of the finger, which had hitherto given me but little uneasiness, began now to be excruciating; and, notwithstanding the use of warm fomentations and poultices, increased to a degree which was almost insupportable. The seat of pain, however, was not the wounded part; but nearly an inch above it, in the second phalanx of the finger. My friend Mr. Brodie saw me late in the evening, in conjunction with Dr. Granville; and as it was supposed that suppuration had begun under the fascia, it was determined to lay it open by a free incision down to the bone, which was accordingly done by Mr. Brodie; and the hand was afterwards exposed to the influence of the steam of boiling water. Scarcely more pus than was sufficient to cover the point of the lancet was evacuated; but by continuing the steaming, at intervals, for several days, the part suppurated freely, and my recovery was completed. At the end of ten days I was again able to renew my professional avocations, although some weeks elapsed before the debility in which I was left by the disease, was entirely overcome. I ought to mention that the middle finger of my left hand, on which a small morsel of the cuticle close to the nail was separated, forming what is vulgarly termed "a hang nail," became inflamed, suppurated, and cast the nail.

The following corollaries are deduced from the facts connected with the origin and progress of this case:—

1st. It is evident that diseases of an inflammatory nature, affecting the serous membranes, even when *unconnected with the puerperal state*, generate a virus, which continues active for some time after the death of the individual; and is capa-

ble, when introduced into the living system by inoculation, of exciting a dangerous degree of irritative fever.

2d. It is probable, however, that a certain predisposition of the body of the dissector may be necessary for the production of this effect.

In my own case, my health was not in the best state at the time of my receiving the scratch, as I had been previously much harrassed both in body and mind, by the extent and nature of my professional duties.

3d. That the effect of the introduction of this virus into wounds, or chops, or abrasions of the cuticle of the hands in dissection, is local inflammation and the production of a similar virus in the part, which, being absorbed into the system, diminishes so greatly the nervous energy, as nearly to destroy the action of the heart ; and, thence, to produce congestions in the vascular trunks highly detrimental to the powers of life, and which prove not unfrequently fatal.

If these corollaries be correct, two questions of great importance present themselves for consideration :—1st. In what manner, independent of care in avoiding wounds, is the influence of this virus to be guarded against in dissection ? 2d. When inoculation has taken place, what plan of treatment is likely to prove most beneficial in overcoming the disease which follows ?

In considering the first query, it is obvious, that no description of gloves, nor any coverings for the hands of a similar nature, can be employed by the dissector ; but I am of opinion, that the hands, if chopped or abraded, might be protected by rubbing them over with oil. It is a well-known fact, that the oil coolies at Tunis are rendered insusceptible of the contagion of plague owing to their bodies being constantly covered with oil, which can be explained only on the principle of non-contact ; and as oil is impermeable by watery fluids, the probability is, that it would secure the chopped or abraded hands of the anatomist, in morbid dissections, on the same principle. But it cannot secure him from being wounded either by the scalpel, the needle, or the edges of fractured or of carious bones, or from the consequent inoculation ; and, thence, the necessity of the second query.

The wounds which are received in morbid dissections are seldom deep, and most frequently do not penetrate through the cutis vera. When this is the case, perhaps the most certain mode of preventing the threatened evil would be, instantly, to cut out the portion of wounded skin with a *clean* scalpel, and to encourage a flow of blood by bathing it with warm

water. Sometimes, however, scratches are received unconsciously, owing to the attention of the dissector being deeply engaged in the investigation of his subject; and the first notice he receives of the injury is from the local inflammation of the wounded part. It is then too late to have recourse to excision: what mode of treatment, therefore, should be adopted? Were I to reason from my own case only, I might consider it sufficient to refer to the treatment which it details; but the Profession has been instructed, by experience, not to depend upon the result of single cases. If the view, however, which has been given of the nature of the attack be correct—if the first effect of the absorption of the virus be to diminish the nervous energy, and, by thus weakening the moving powers of the blood, to permit congestions in the trunks of the vascular system,—it is evident, that, unless the balance of the circulation be restored, the functions of life cannot be continued.—Reaction may take place by the powers of the habit; or the congestion may be removed by bleeding; or it may be overcome by reaction induced by rousing the nervous system by artificial stimuli, as in the case before us. If we trust to the powers of the habit, it is impossible to calculate upon the consequences: the resulting fever may be sufficient to endanger life; or organic mischief may occur, which would ultimately destroy it; and it is unnecessary, therefore, to reason upon the probable effects of the vis medicatrix in cases of this description. It only remains, therefore, to estimate the comparative advantages of bleeding and of stimuli. The opening a vein in the arm will undoubtedly relieve congestions in the vascular trunks within the thorax and abdomen; but if these congestions have arisen from the nervous energy being diminished, by the introduction into the habit of a virus operating as a powerful sedative, it may be doubtful whether, after they have been thus relieved, the balance of the circulation will be maintained. If the moving power of the vascular system depends on nervous energy, the mere unloading the great vascular trunks, in which the blood has accumulated from the action of the heart being unequal to propel it onwards, owing to a paralysis, as it were, of the nerves, is not likely to restore that power; and, therefore, we must conclude, that the lancet is less efficient than stimuli, which are calculated to rouse and maintain the nervous energy. The result of the practice which has been generally employed in these cases, might be brought forward to determine the question; but the records of it are too scanty to admit of a satisfactory inference being drawn from it. In my own case, in which there was evident-

ly no injury either to a nerve or a vein, the beneficial effects of the stimulating plan, in the first instance, followed by active purgation, was decisive; and my friend Dr. Granville, to whose skill, judgment, and kind attention, I attribute my recovery, has found it equally so in several cases which have since occurred, and come under his care. It is, at all events, worthy of being tried by the test of experience.

Before closing this communication, I shall mention a curious physiological fact, of which I have hitherto been unable to obtain any satisfactory explanation. The cuticle of the finger, which was scratched in the dissection, having separated, owing to the inflammation and the use of the steam-bath, a great increase of sensibility followed; and along with this, an extraordinary *idea of extension* was communicated. Thus, in touching my hair, or my skin, each hair felt like a rope in magnitude, and the minute and almost invisible fissures of the skin became obvious and highly perceptible to the touch. The increased sensibility is easily explained; but in what manner are we to account for the idea of increased extension?—*London Medical Repository.*

Extract of Nux Vomica in Paralysis.

Marguerite M., aged fifty-six years, had suffered much from seeing this lady almost die of a cancer of the tongue. She had attended her for nearly thirty years. Some time previous to this, Marguerite had become affected with a small troublesome ulcer, situated on the right side of the tongue, exactly resembling, even as to situation, that which in her mistress had degenerated into cancer. The mind of this woman was very highly excited by it. The juice of herbs, emollient applications, soothing her mind, and the extraction of some carious teeth, were the means which we employed, and with success.

The small ulcer became cicatrized; nevertheless, she continued to abandon herself to despair. She had from the first some difficulty in assisting herself; but, as her despondency became more severe than ever, we could perceive her, distinctly, lose by degrees the power of motion first in the lower extremities, and then in the upper: neither did the exhibition of bark, musk, strong jellies, nor of the preparations of steel, nor stimulating frictions, in the least prevent or check the pro-

gress of this paralysis. In this most miserable condition she entered the hospital.

We consoled her in every possible manner, and cheered her with the hope of recovery, pointing out to her the use of the extract of nux vomica, as an infallible mean of having that hope realised. We at first gave her but half a grain for a dose, but gradually increased it, in very small proportions, until she took four grains night and morning; at which time, without its being the effect of any accident, a tetanic rigidity, accompanied with strong spasms, involuntarily and frequently repeated, affecting the whole body, manifested themselves an hour after taking of the medicine. She now already showed considerable signs of relief; she could lift her legs, and support herself upon them for some minutes. We gradually augmented the dose of the medicine, increasing it until she took twelve, fifteen, and at last eighteen grains, during the twenty-four hours. She, however, never took the medicine without experiencing immediately a very sensible effect. Sometimes she had that rigidity and general spasm, joined with slight delirium, or a species of coma, not however alarming; all which disappeared in the space of fourteen or fifteen minutes. She continued the nux vomica for nearly five months, having been interrupted in its use but five or six days, on account of a gastric irritation; which, however, a few grains of ipecacuanha and two purgatives relieved. It procured a re-establishment to her health, and a recovery from a severe paralysis of both sense and motion, which had nearly proved fatal.

It is now four years since this recovery took place, and without being followed by any relapse.

We have pushed the dose of this medicine even to the extent of fifteen grains in a young girl, fifteen years of age, who in consequence of a severe fright the preceding summer, was seized with complete paralysis of the bladder, the rectum, and lower extremities. Sensation and motion of these parts were quite destroyed. After taking the medicine, she experienced the very same effects as those described above; and, when her dose was increased to eight grains, she could retain her urine, and fæces, and move her lower extremities. She was afterwards enabled, though but slowly, to sustain herself upon her legs, and take a few paces, by laying hold of the bed-posts; taking at this time ten or twelve grains of the medicine. She could walk tolerably well with assistance when she left the hospital, since which time we have had frequent opportunities

of seeing her. Her legs are still weak, but she can now walk with the help of a stick as her only support.

We have not yet used the medicine with such decided success in any other cases. We have always been unsuccessful in those cases which were caused by apoplexy. In one case only, which happened in an old man, seventy years of age, of a full habit, who was brought to the hospital with a severe apoplectic attack, which we treated with copious blood-letting and cupping, did we succeed in restoring the sensation and motion of the right arm. The dose in this case was twenty grains, which was continued with perseverance, and always excited strong spasms in the whole of the diseased limb. We are, however, convinced that, if the treatment in this case was followed with success, it was owing to the previous copious depletion. The bad success, also, which we have always experienced in those cases not particularly favourable, makes this conclusion appear the more correct.

These three cases have convinced us of what others had already witnessed,—viz. that there are cases of palsy, which, although very severe, may yet be cured by the extract of the nux vomica. But we ourselves are led to conclude, in analyzing the result of our experience, that they are cases which depend upon a weakness or mobility of the nervous system, and which are not caused by any organic change followed by an attack of apoplexy, or any other disease which implicates either the brain or its appendages. With respect to paralyses arising from a state so distressing, they are generally incurable, and the extract of nux vomica affords no relief, because it remains without acting on the cause.—*Journal Generale.*

Pelletan, Bally and others *on Acupuncture.*

Since we had occasion to notice Mr. Churchill's Treatise on Acupuncturation, several important communications on the subject have appeared in this Journal. Acupuncture has recently attracted much attention in Paris; and, as our readers will perceive from the articles the substance of which we are now about to place before them, the Physicians of that city have employed it in several disorders in which it was hitherto looked upon as an inapplicable, if not a dangerous experiment. The journals now before us contain much inform-

ation on the subject; this we shall condense into one article, for the consideration of the Profession in this country.

The sudden and mysterious relief, of which acupuncturation has so often been found productive, will probably ere very long be explained, and it does not seem unlikely that its explanation will throw some light upon the hitherto obscure physiology of the nerves. M. Jules Cloquet thinks, that during this process there is a fluid disengaged, which he compares to the electric, and to which he gives the name of the nervous fluid. He remarked that the needles employed are blackened when used in the living, though not in the dead subject. M. Pelletan says, that the presence of this fluid is indicated by the galvanometer. It oxidises the needles and gives them a blue colour. Iron needles are recommended, as being the best conductors of this fluid.

The elaborate paper on the subject, by M. Pelletan fils, in the *Revue Medicale* for January, embraces the history, the effects, and the theory of acupuncture. The Professor refers to the Latin Dissertation of William Ten Rhyme, and to the *Amenitates* of Kempfer; but says he has not had any very exact account of its recent introduction into England 'par un medecin Chinois,' who, we suppose, is Mr. Churchill. In the year 1811, a case was communicated by Berloiz to the Society of Medicine of Paris, of a woman affected with 'a severe nervous remittent fever,' who had been habitually relieved by acupuncturation of the epigastrium: so long as the needle remained inserted, the patient felt perfectly relieved: at last an accident completely cured her; for a shorter needle than usual being employed, it could not be withdrawn. The latter peculiarity of this case did not, however, excite much attention. A woman affected for eighteen months with continual hiccup, and who was sinking under marasmus, was relieved for four-and-twenty hours by the first needle, and subsequently cured, under the care of M. Haime.

M. Beclard made a number of experiments, in order to ascertain the safety of employing acupuncture in different situations, and found that the needles might be inserted without danger in the most important organs. He observed, for example, that a large artery, or a nerve, might be pricked without either hæmorrhage or pain being the consequence. M. Bretonneau made similar experiments on living animals, and found that even the large cavities of the body, the brain, the womb, and the heart, might be pierced by the needles used in acupuncture without dangerous consequences. MM. Velpeau and Meyranx inserted the needles into the lungs,

liver, heart, and intestinal canal of several dogs, without remarking any more than occasionally some temporary derangement.

These preliminary experiments being stated, we next notice the practical purposes to which the writers before us have applied the operation.

We need not advert to the employment of acupuncture in this country; the extent to which it has been tried must be well known to the readers of this Journal. We therefore proceed, at once, to state briefly the facts which have been adduced on the subject by our neighbours, the French Practitioners. Berloiz and Haime resorted to it with much success in rheumatism and spasm. M. Demours cured several cases of ophthalmia by means of it and dry cupping. M. Sarlandiere employed it in several diseases; and in some instances, after having introduced the needle, made it the conductor to a discharge of the electric fluid. 'M. Bretonneau cured a hiccup of long duration, by the introduction of a needle into the coats of the stomach!' M. Jules Cloquet more recently made very numerous trials of acupuncture, at the hospital St. Louis, where he had great opportunities of ascertaining its effects, both in a physical and in a therapeutical point of view. On account of the baths, with which this hospital is provided, and their reputation in the cure or alleviation of painful affections, patients labouring under them are sufficiently numerous in its wards. M. J. Cloquet, in the course of his trials of the remedy, conceived that, if the needles were allowed to remain inserted in the affected part much longer than they usually were by his predecessors, he should obtain more uniformly beneficial effects from the operation. He, moreover, entertaining the idea that a fluid escaped from the needles, sometimes established a galvanic circuit with a conducting wire, communicating, at one end, with the needles inserted into the affected part, and at the other end, with the mouth. Sometimes he plunged both extremities of the conductor in a glass of salt water. Of the phenomena observed on these occasions, we shall have to speak in the sequel; at present we proceed to notice the therapeutical effects of this operation.

M. Pelletan observes that the puncture, during the presence of the needle, is often surrounded by an areola of various forms; the form having, however, no relation to the local pain; —that the pain for which it is employed rarely ceases sooner than five or six minutes;—that it is necessary to allow the needle to remain, generally, from ten to twenty minutes, and sometimes even several hours;—that the diminution of the

pain is always accompanied or followed, and occasionally preceded, by a sensation of numbness ;—that, when the pain returns, after having ceased, in consequence of the operation, it always is less severe, and disappears sooner on the repetition of acupuncture ;—that the beneficial effect of the operation is always more marked the nearer it is performed to the nervous trunks going to the affected parts, on the side of their origin ;—that the pain disappears, first in the remote extremities of the nerves, and lastly in the trunks ;—and that, although acupuncture may be innocently employed on arterial, venous, or nervous trunks, they ought to be avoided ; provided this be done, very fine needles may be inserted, with advantage, at great depths, in the abdomen and thorax, without inconvenience.

M. Pelletan has seen acupuncture successfully practised ; —1st, in violent neuralgia ; 2d, in rheumatism ; 3d, in accidental contusions, and anomalous pains ; and, 4th, in chronic affections. M. J. Cloquet has performed acupuncture on about three hundred patients in the hospital St. Louis, and from amongst these he reckons only twenty in whom the practice had no effect : it happened in a few that the pains were increased by it. No accident occurred from the operation.

M. Bally first employed acupuncture at the hospital ' de la Pitie,' in cases of rheumatism. He considers that when inflammatory fever accompanies the disease, the practice is ineffectual ; in other circumstances, he has generally found it successful. In neuralgic affections, he conceives it to be the best remedy we possess ; and his pupil, Dr. Meyranx, details several cases wherein it either produced an immediate and permanent cure, or much relief. In two or three cases, however, it proved inefficacious. M. Bally practised the operation in two cases of hemiplegia, on the paralytic limbs, without any advantage. ' The introduction of the needles seemed to possess a most decided influence in cephalalgia and hemicrania, when these complaints seemed unconnected with a morbid state of the brain or its membranes, or of the bones of the cranium.' In the local pains resulting from venereal affections, it was either inefficacious or gave but momentary relief. Acupuncture was resorted to in a case of chronic gastro-enteritis, but without benefit. In a case of pleurodynia, occurring after pleuritis for which the patient had been blooded generally and locally, acupuncturation was found beneficial. ' Three days after the patient considered himself cured of the pleuritis, the pain returned without occasioning general symptoms. The respiration was a little embarrassed : the

other functions were not disturbed. Three needles inserted into the chest, at the part where the pain ceased; and a second application of the needles was followed by the disappearance of the cough which returned with the pain.' The following case is interesting :—

'Denios, of a nervous temperament, has had, for five years, all the symptoms of chronic inflammation of the pericardium, or of the surface of the heart. Repeated blood-lettings, the application of several blisters, and cooling drinks, with the usual concomitants of the antiphlogistic regimen, had been practised without relief. At this time he complained of acute pain in the left breast, and under the sternum of the same side, and of continual anxiety at the præcordium. The stethoscope gave no pathognomonic sign. The action of the heart was without noise: the pulse was frequent, small, and sometimes tremulous.

'M. Bally having proposed acupuncture to the patient, I inserted obliquely three needles, to the depth of half an inch, in the thoracic parietes over the heart. A very marked areola formed around each needle; the pulse became smaller and feebler; the face paler, and expressive of more anxiety than usual: I dreaded syncope; and was about to withdraw the needles, when the patient expressed himself much relieved, and that the puncture gave him pleasure. The needles were allowed to remain for an hour and a half; at the end of which time the pain was less, and the respiration much more easy. The following day he remained better, and desired a repetition of the operation. M. Beclard, who accompanied M. Bally at this visit, introduced four needles in the region of the heart: one of these penetrated as far as the left lung. It was evident that it had reached this organ, or the pericardium itself, from the double motion communicated to the needle by the heart, and the acute pain felt by the patient on inspiration. The needles were allowed to remain only for a quarter of an hour. This second operation did not relieve the patient so much as the first; however, he acknowledged to M. Bally that the pain in the region of the heart was much more supportable since the employment of acupuncture; but that one of the last needles had occasioned so much pain that he would not again submit to the practice."—*London Med. Repository.*

Mr. Jones' *Observations on the Pathology of Debility.*

Debility—is the effect of diminished nervous energy; and, although generally a symptom of disease, might be considered as arising from two causes: 1, protracted inordinate arterial action; 2, venous congestion.

1. *Protracted inordinate arterial action.*—The various actions of life are necessarily accompanied with an expenditure of nervous energy, and nature has wisely guarded against the consequences of such expenditure, by ordaining sleep as the means of replenishment. Thus the vigour of the system, which is diminished by our daily avocations, is restored by the balmy influence of slumber. If, when in health, an individual has been exposed to great exhaustion from inordinate exertion, in proportion to the unusual expenditure of the animal powers, must the duration of his slumbers be protracted, in order to restore the system to its wonted vigour; although this is greatly under the influence of habit, and therefore some persons require less sleep than others to produce the same effect. If the increased action arises from the occurrence of fever, the debilitating cause operates during sleep, as well as when awake; and consequently there is no replenishment of vigour. The debility progressively increases, either till the fever subsides or there is a total expenditure of the powers of life.

Debility is often connected with an irritable state of the system, which has a considerable influence in modifying disease. Irritability, in reference to disease, might be defined—a peculiar susceptibility for impressions, by which pain is readily induced, or the system unusually agitated by the application of stimuli. Thus, some individuals are affected more easily than others by mental impressions. Local injuries, which in some persons will only be productive of the most trifling symptoms, will in others cause violent inflammation and its consequences, by which the system might become sympathetically affected with fever, convulsions, &c. This state may depend on constitutional predisposition, or it may be the effect of disease. It is invariably the consequence of habitual intemperance. About the time of puberty in females, it disposes to hysteria; it particularly characterises the period of infancy, and modifies most of the diseases to which children are subject. The irritation which accompanies local injuries is evidently produced by a determination of arterial blood to the part; and we find that, in proportion to the arterial action ex-

cited, is the accompanying susceptibility for pain. If, then, different degrees of morbid action, from slight local irritation to positive inflammation are owing to corresponding degrees of impetus of arterial blood, it seems fair to infer that the morbid irritability of the system, which so often accompanies debility, depends also on arterial action. That this is the case in those diseases peculiar to infancy, is proved by that period being particularly marked by arterial plethora ; and likewise, in adults, morbid irritability is invariably accompanied by an accelerated pulse. Females appear more subject to this state of the system than males. Their nervous system is doubtless more delicately constructed, which renders it naturally more susceptible of impressions, and more active in deriving its peculiar principle from the blood ; their pulse is quicker than that of the males, and we find that most of their diseases are greatly modified by irritability.

Morbid irritability, therefore may be considered as partly constitutional, connected with weak stamina ; and partly the effect of increased expenditure of nervous energy, in connexion with arterial action, in whatever way produced, and which becomes evolved in greater proportions than is consistent with the healthy energies of the system.

In children, debility is not a consequence of arterial plethora, because the increased production of nervous energy attending such a state is instrumental in promoting the growth of the body, and consequently adds to its vigour.

2. *Debility from Congestion.*—This is the most frequent cause of debility. It has already been explained in what manner it has a direct tendency to produce this effect,—viz. by impeding the due development of nervous energy. When such impediment is suddenly induced in the principal internal organs, idiopathic fever is the consequence, in which the system is attacked by two formidable enemies: the one depriving it of its energies, by increased arterial action; the other preventing their re-production, by congestion. If, then, venous congestion necessarily produces debility, and is the most frequent cause of such a state, this important fact becomes an admirable guide in the treatment of a very numerous class of diseases, and cannot fail of being of great practical utility.—*London Med. and Phys. Journal.*